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HERBERT S. LANGFELD

PRINCETON UNIVERSITY

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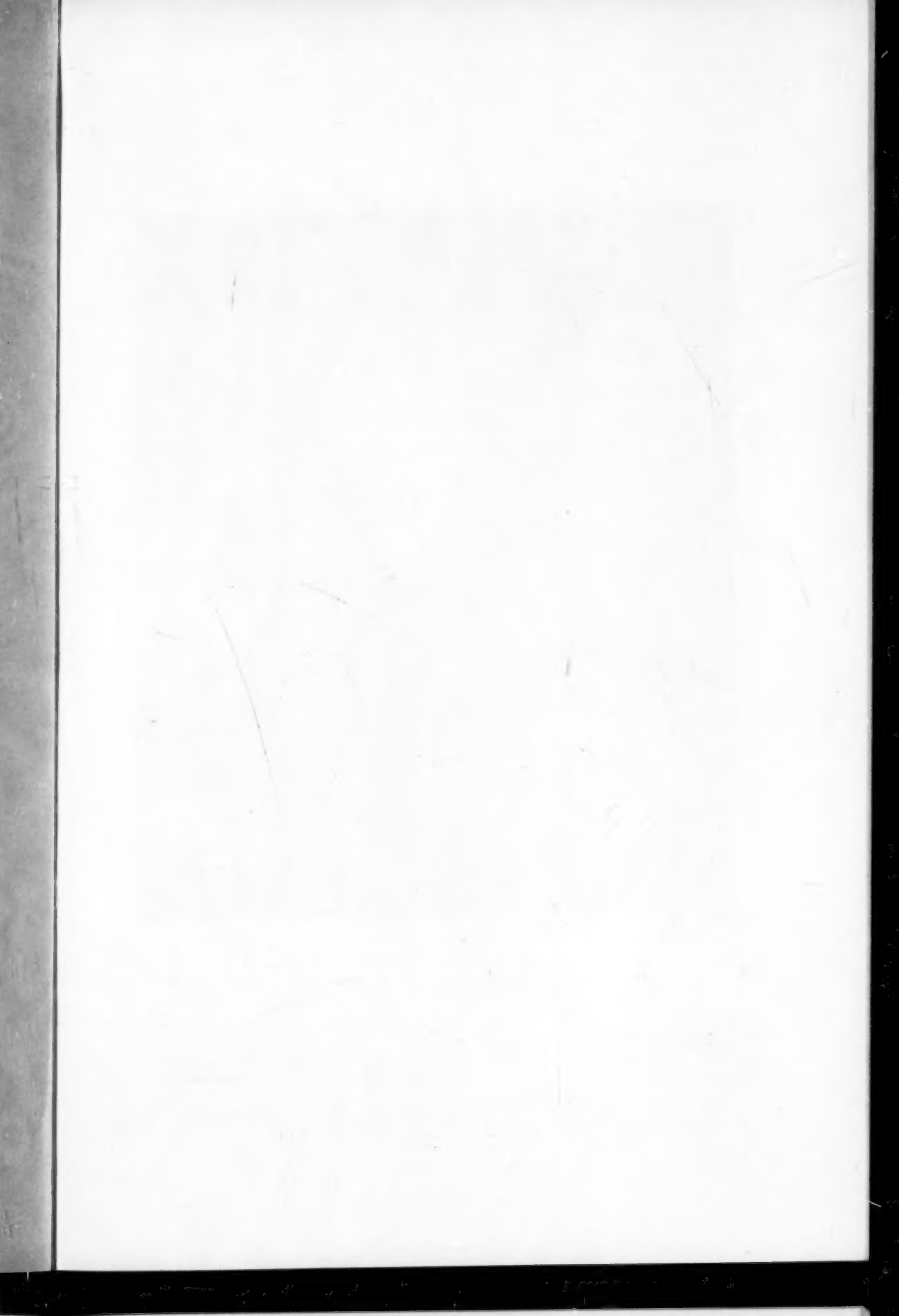
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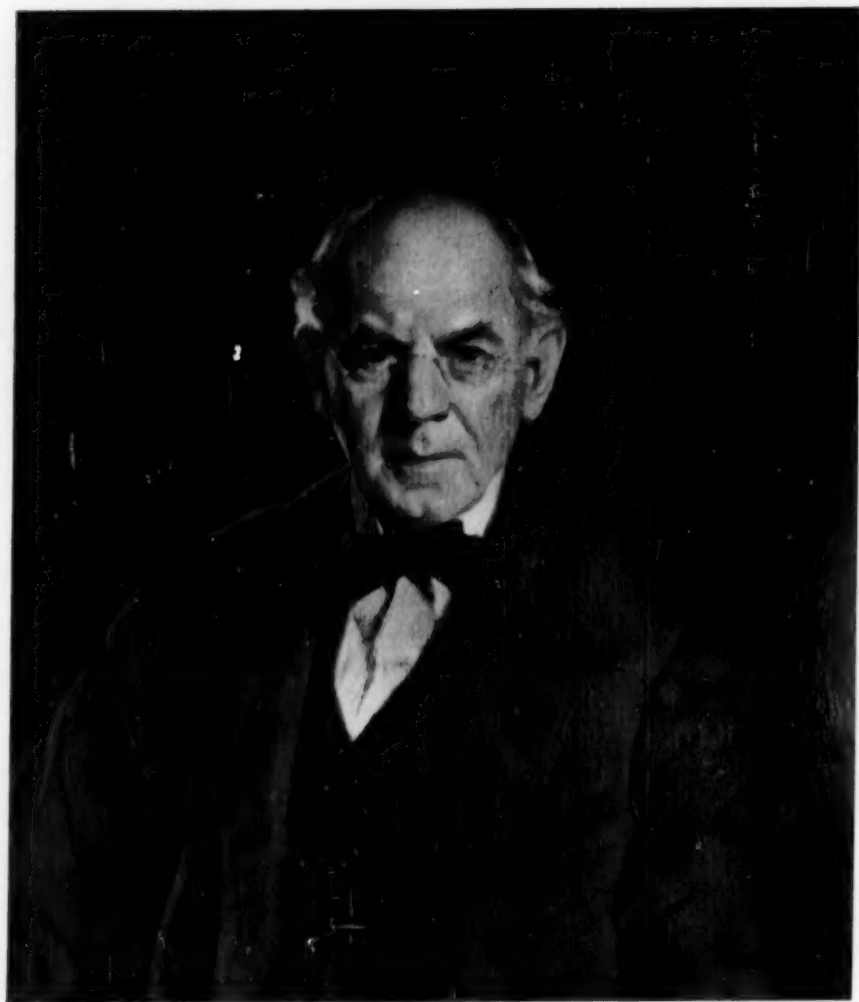
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J. Mark Cattell

THE PSYCHOLOGICAL REVIEW

JAMES McKEEN CATTELL

1860-1944

In the history of American psychology very few figures are so outstanding as that of James McKeen Cattell whose long and active life has just come to a close. He did not, indeed, belong to the first generation of American scientific psychologists—consisting mainly of William James, G. Stanley Hall and George Trumbull Ladd—but he was probably the most influential of the second generation which included Titchener, Münsterberg, James Mark Baldwin, Jastrow, Sanford, and Scripture, with others coming along just a little later. Though Cattell was not a systematist and did not found a school in that sense, he was the leader in what became a widespread and distinctive movement in American psychology. His interest from the very outset of his career was in introducing quantitative methods into psychology and especially in using such methods for the measurement of individual differences.

Cattell graduated in 1880 from Lafayette College, of which his father was the president. His undergraduate interests had centered largely on literature. His first step toward a professional career, however, was to go to Europe for the study of philosophy. He heard Wundt lecture at Leipzig and Lotze at Göttingen and was much impressed by

both of these men. A paper on Lotze won for Cattell a fellowship in philosophy at Johns Hopkins, where he spent the year of 1882-83, with John Dewey and Joseph Jastrow as fellow students. It was during this year that Stanley Hall set up his psychological laboratory at Johns Hopkins, with some assistance from this group of students, and it was there, apparently, that Cattell began his "psychometric investigations," concerned with the timing of various mental processes. He took his data and his designs for improved apparatus back to Germany the following year and remained in Wundt's laboratory for the three years, 1883-1886, being for part of this time Wundt's first laboratory assistant.

From the outset Cattell seems to have been impressed with the variability of human performance and the consequent need for long series of observations in order to reach reliable results. He set up his apparatus in his own rooms at Leipzig so that he could work longer hours than Wundt permitted in the laboratory, and carried out an extraordinarily thorough and extensive study of reaction times, ranging all the way from the simple reaction through the reactions with discrimination and choice up to free and controlled association. Reaction time was of course no novelty in the Leipzig laboratory, being in fact a line of experiment on which Wundt was pinning great hopes. Cattell's conception of reaction time studies, however,

James McKeen Cattell was the joint founder with James Mark Baldwin of *THE PSYCHOLOGICAL REVIEW* in 1894. He was co-editor of the *Review* with Baldwin until 1904, each editing it on alternate years. THE EDITOR.

differed radically from that of Wundt. Wundt hoped by variation of the experiment, with certain introspective controls, to tease out the time constants for elementary mental processes such as perception, choice and association. Cattell found that he could not himself carry out the required introspections and subjective controls, and he came to doubt the ability of others to do so. It seemed to him that the simple reaction became with practice a "prepared reflex" and that in the more complex reactions the constituent processes overlapped in time and so could not be measured. Yet the reaction time experiment, he still held, was of great value as a tool for determining the speed and difficulty of many everyday mental processes. He could show, for example, that the time required to read a short familiar word was no greater than that required to read a single letter, so that the practice then coming into vogue of teaching the child to read whole words before the single letters had a scientific basis. Without pretending to analyze the complex processes into their elements, Cattell used the total reaction time obtained under various conditions for studying attention, fatigue and practice, for comparing the legibility of the different letters of the alphabet and for many other practical and scientific purposes.

Cattell's Leipzig studies were all concerned with time, but they were not limited to reaction time (3). He used his 'fall tachistoscope' also for determining the exposure time necessary for perceiving colors, pictures, letters and words (2). He also made an interesting use of a serial exposure apparatus (1)—a type of experiment which has not been followed up as much as it deserves.

Cattell continued to use the reaction time method in important later studies (7) and directed quite a number of his Columbia students in similar work.

Not during his years at Leipzig, but

shortly afterward, Cattell carried on extensive work in another of the classical fields of experimental psychology, psychophysics, (5, 6). Here, as well as in reaction time, he broke away from the older view of these experiments as being concerned with the measurement of consciousness and substituted a more objective and operational conception. Experiments using the method of right and wrong cases or of constant stimuli, for example, are not directed operationally toward the measurement of intensity of sensation. They are experiments in observation and judgment and the results come out as measurements of the error of observation. Psychophysics, accordingly, should be conceived as a study of accuracy of observation under different conditions—a study of obvious practical importance. It seemed to Cattell more in accordance with the theory of probability (a theory in which he took much interest) to expect the error of observation to increase as the square root of the observed magnitude, rather than in direct proportion to that magnitude as asserted in Weber's law. As a matter of fact, the data usually come out between these two formulas. That is, the error of observation usually increases less rapidly than Weber's law would predict but more rapidly than predicted by Cattell's square root law.

A little later (12), by combining his interests in psychophysics and reaction time, Cattell invented a new psychophysical method, the discrimination time method for indicating the difference between magnitudes or qualities—the larger the effective difference, the quicker the discrimination.

Though differing with Wundt on some matters of theory, Cattell always retained a warm personal affection for his master and a high respect for his services as a founder of experimental psychology. After leaving Leipzig Cattell soon came into personal contact with

Francis Galton—"the greatest man whom I have known"—and was confirmed by Galton in his own long-held view that the measurement of individual differences would be one of the most fertile fields for the new psychology. Cattell was perhaps the first (1890) to use the term mental tests, and he thus expressed his high hopes regarding them:

Psychology cannot attain the certainty and exactness of the physical sciences, unless it rests on a foundation of experiment and measurement. A step in this direction could be made by applying a series of mental tests and measurements to a large number of individuals. The results would be of considerable scientific value in discovering the constancy of mental processes, their interdependence, and their variation under different circumstances. Individuals, besides, would find their tests interesting, and, perhaps, useful in regard to training, mode of life or indication of disease (4, p. 373).

At this time he described a series of ten tests which he apparently was using at the University of Pennsylvania. Shortly afterwards, at Columbia, he developed a more extensive list, known for many years as the Freshman Tests, though they had nothing to do with the admission of freshmen to college (10). They were given to 50 or more volunteers from each successive freshman class, in order to obtain data for the study of individual differences and the factors on which the differences depend. When the sample of freshmen had grown to a sufficient size, the Pearson method of studying correlation having meanwhile become available, these data were subjected to correlational analysis. Though the low correlations obtained were surprising and rather disappointing to Cattell—they suffered from attenuation, as Spearman soon pointed out—this study has considerable historical importance in the development

of the correlational method in psychology (28).

Cattell's plan of testing separate functions—the senses, quickness of movement, perception of time, memory, imagery, etc.—was rather left behind with the appearance of Binet's method of testing intelligence, though it is more in line with recent efforts to develop tests for specific mental abilities.

Reaction time, psychophysics, and tests were thus the main lines of Cattell's early researches. A minor extension of his work on errors of observation is of historic interest as being probably the first study of the reliability of testimony. He wrote:

... we do not know how likely it is that a piece of testimony is true, or how the degree of probability varies under different conditions. If we could learn this by experiment the result would be a contribution to psychology, and would at the same time have certain important practical applications (8, p. 761).

His experiment consisted in asking college students questions about distances on the campus, the weather a week before, the dates of certain historical events, etc. He found wide individual variation in the students' answers. In some cases the average of the answers was close to the truth while in other cases there was a large constant error.

When students were asked what was said during the first two minutes of the lecture in the same course given one week before, the accounts were such that the lecturer might prefer not to have them recorded. From the testimony of the students it would appear that two minutes sufficed to cover a large range of psychological and other subjects, and to make many statements of an extraordinary character (8, p. 764).

A major contribution, besides those already mentioned, was the invention of the order of merit or ranking method for use both in psychophysics and in

aesthetics and other judgments of value. Cattell first employed it in a psychophysical problem. Having prepared a series of over 200 shades of gray ranging by imperceptible steps from black to white, he asked his subjects to arrange them as well as possible in order of brightness. The observer's errors could be determined by reference to the objective scale of brightness (12). He soon extended the use of the method to the broad field of value judgments. The first value considered was the scientific standing of American men of science, that is, the relative standing of the men in any given branch of science. In the case of psychology, for example (14), he first prepared a list of all who could be regarded as scientific psychologists, and then induced ten leading psychologists to serve as judges and rank the listed individuals in order of scientific merit. The ten judges worked independently and Cattell combined their rankings and computed the average position assigned to each individual, with the variation from judge to judge. The average ranks were not published till thirty years later (16, 5th edition, 1933, pp. 1269 ff.; 24, p. 11). Meanwhile, however, accepting the average ranks as furnishing an approximation to a true order of merit, he was able to use the results in two ways. By comparing each judge's arrangement with the average of all, he had an estimate of the accuracy of each judge. From the average ranks combined with other data he was able to make what might be called an ecological study of *Homo scientificus Americanus* with respect to parentage, place of birth and of education, and present geographical distribution (17); and by repeating this study at about seven-year intervals he brought to light changes and trends of considerable interest (18). The ranking method was quickly applied by his students and colleagues to a great variety of value judgments related

to literature, education and business. Cattell himself made some progress in the difficult task of working out the relations between rank order and quantitative measurement.

As was said before, Cattell did not found a school of psychological theory. He was open-minded towards all kinds of psychological research and application, provided they were serious and scientific, and was willing to have many sorts of experiment going on in his laboratory. His own preference was definitely for the objective type of experiment. He no doubt prepared the way for behaviorism and felt considerable respect for it when it emerged but he was not willing to rule out of the science those who preferred the introspective method. There was a great difference between the types of work done in his laboratory and in Titchener's, yet he had a high respect for Titchener as a scientist. Certain statements in Cattell's address at the St. Louis World's Fair in 1904 have often been quoted and deserve to be quoted again:

Sciences are not immutable species, but developing organisms. Their fundamental conceptions and methods at any period can only be approached by a research into work actually accomplished. . . . The task has been assigned to me of considering the scope, conceptions and methods of psychology, and it is my business to define the field of psychology or to acknowledge my inability to do so. I must choose the latter alternative. I can only say that psychology is what the psychologist is interested in *qua* psychologist. . . . I am not convinced that psychology should be limited to the study of consciousness as such. . . . I admire . . . the ever-increasing acuteness of introspective analysis . . . but the positive scientific results are small in quantity when compared with the objective experimental work accomplished in the past fifty years. There is no conflict between introspective analysis and objective experiment—on the contrary, they should and do con-

tinually coöperate. . . . Let us take a broad outlook and be liberal in our appreciation. . . . As I claim for psychology the freedom of the universe in its subject-matter, so I believe that every method of science can be used by the psychologist. The two great achievements of science have been the elaboration of the quantitative method on the one hand and of the genetic method on the other. . . . It would be an irreparable limitation if either of these methods did not apply in psychology. . . . I see no reason why the application of systematized knowledge to the control of human nature may not in the course of the present century accomplish results commensurate with the nineteenth century applications of physical science to the material world. . . . In the end there will be not only a science but also a profession of psychology (15, pp. 176, 179, 180, 182, 186).

If we try to bring before us the young Cattell who emerged from Leipzig in 1886 with the degree of Doctor of Philosophy, the picture is one of a man of great initiative and energy, eager for large enterprises, and filled with missionary zeal for the advancement of a psychology which should be experimental, quantitative and practical, with great emphasis on the study of individual differences. For two years this young man divided his efforts between England and America, working in Galton's Anthropometric Laboratory in London, lecturing in Cambridge University and making a start toward a laboratory there, and lecturing also at Bryn Mawr College and the University of Pennsylvania. In 1888 he became a professor at the latter institution, and he was always proud of the fact that he was Professor of Psychology and that this was the first professorship of psychology as distinguished from philosophy that was ever established anywhere (23). Cattell was never hostile in the least to philosophy or philosophers; he was especially appreciative of John Dewey; but he be-

lieved that psychology should align itself with the sciences. His laboratory at Pennsylvania was the first one to provide not only for research but also for the initiation of the college student into the methods of experimental psychology. After a few years at Pennsylvania he accepted a call from Columbia University where he started the laboratory in 1891. Here his influence was felt by many students, including over fifty who took their doctor's degree with him up to 1917 when his connection with the university ceased. The majority of these graduates became active psychologists in various parts of the country, and they look back to Cattell with loyalty and with gratitude for his helpful stimulation and guidance.

Besides his research and teaching, Cattell's enterprising activity branched out in several directions. He took quite an interest in designing improved and simplified forms of psychological apparatus and in having it manufactured in the laboratory shop, so making a contribution, as he felt, to the development of experimental psychology in the laboratories that were springing up throughout the country. This was one of his earliest enterprises, continued for a couple of decades at Columbia.

Another early enterprise was the editing and publishing of scientific journals, and this continued as a major activity for the rest of his life. In 1894 he joined forces with James Mark Baldwin, then professor at Princeton, in establishing the *PSYCHOLOGICAL REVIEW* series. In the same year he acquired the weekly journal, *Science*, which had just suspended publication because of financial difficulties. He secured the coöperation of an eminent editorial board while taking on himself the arduous tasks of managing editor and business manager. He set up an editorial and publishing office at his country home on the mountain top in Garrison, N. Y., fifty miles from

the university, and with the able assistance of Josephine Owen Cattell, his wife, produced an extremely well-edited journal which after a few years became a financial success and was accepted as an indispensable service to American science. A few years later he similarly took over the *Popular Science Monthly* and made a success of it, later renaming it the *Scientific Monthly*, and in 1915 he started a comprehensive educational weekly, *School and Society*. All this editorial work took him away from active research, but such services of a psychologist to the causes of science and education redounded greatly to the credit of psychology.

The same was true of his active participation in the general organization of American scientific men. First we should notice that he was one of the small group that started the American Psychological Association in 1892 (21). He was a member of the Council from the beginning, Secretary the third year and President the fourth year (9). Soon after coming to Columbia he became a member of the New York Academy of Sciences and soon induced the Academy to set up a Section of Anthropology and Psychology, so winning recognition for our science from this local scientific body. He was President of the New York Academy in 1902 and set forth in his presidential address his ideas on the appropriate organizational scheme for American science. He said:

The organization of science in America toward which I believe we are moving is this: We shall have a national society for each of the sciences; these societies will be affiliated and will form the American Association for the Advancement of Science. . . . Our national societies will consist of local sections, and these sections will unite to form an academy of sciences. . . . This kind of organization may appear to be almost too logical for a world that is somewhat careless of logic, but it is in part already realized (13, p. 972, 973).

In accordance with this idea of local branches of the national societies, Cattell had already in 1900 secured permission from the American Psychological Association to establish a New York Branch, which maintained a continuous and useful existence till it expanded to become the Eastern Psychological Association of today.

Cattell's interest in the American Association for the Advancement of Science (the A.A.A.S.) evidently began very early, for we find him in 1898 Vice-President of that Association and Chairman of Section H, then the Section of Anthropology but soon to become for many years the Section of Anthropology and Psychology. His vice-presidential address at that time made a definite claim for the recognition of psychology as a science (11). He said:

From our present point of view science in its history appears to have followed a necessary course. The phenomena of the physical world are stable and readily subject to experiment and measurement; their control is essential to material progress. It is therefore no wonder that the physical sciences should have preceded the biological sciences in their development. Far more complex, transient and inaccessible to experiment even than the phenomena of living beings are men, they themselves and their deeds—sciences of these things must come later. . . . Psychology has become an integral part of modern science; it gives and takes with a free hand. A parvenu among the sciences, it is self-conscious and knows its obligations and its limitations; but its position in the body scientific is henceforth secure. . . . When we regard the fifty years of this Association or the century now ending, we cannot fail to see that it has been an era of science. . . . The older sciences have been reformed and new departments have been established. But amid all this scientific progress nothing has been more notable—at least from my own partial point of view—than the development of psychology into a science rivaling in activity and fruitfulness the other great sciences.

From 1900 on Cattell's main interests were probably his journals, especially *Science*, his directory of men of science along with his already mentioned studies of these men, and the A.A.A.S. In 1900 *Science* became the official medium for the Association, greatly to the advantage of both, and from that time till the end of his life he was a leader in the Association and probably more influential than anyone else in its affairs (26). He was Vice-President again in 1913, this time for the Section of Education (20), and President of the Association in 1924 (22). He was the first psychologist to receive this distinguished honor, as he had also been the first (in 1901) to be admitted to the National Academy of Sciences. As an active member of the National Academy he was influential in building up the representation of psychology in that body. With his extremely wide acquaintance among scientific men and his varied services to American science in general, as well as by his direct efforts in behalf of psychology, he undoubtedly contributed more than any other one man to win recognition for our science among the group of natural sciences.

Promotion of applied psychology was one of Cattell's ambitions from the very beginning of his career, and one which he emphasized repeatedly in his addresses and writings. He encouraged his students to pioneer in finding applications to education, industry and medicine. Coupled with this desire to make psychology a force for the betterment of mankind was a strong democratic spirit which made him resentful of the necessity of appealing humbly to wealthy donors and foundations, or even to the Government, for the support of scientific research. He pointed to the enormous economic gain resulting from research and urged that a fraction, if only a small fraction, of this gain ought to be turned over to the scientists as a matter of

right and of public policy for the support of further research. It was quite in line with these predilections that he organized the Psychological Corporation in 1921, putting into it funds from his own pocket and securing a liberal charter which permits the Corporation to earn money by applying psychology but provides that a large share of the profits shall be plowed in for further research. In spite of his other responsibilities he helped greatly to direct the policy of the Corporation during its early years of struggling existence, and when it began to have some financial success he turned his own stock into a fund to be used for the support of research in applied psychology.

Even yet we have not mentioned all of Cattell's organizational activities. One of the most important during his last twenty years was an active participation in the development of Science Service. He contributed much to the success of this effort to improve newspaper coverage of scientific events and discoveries, and thus to bring science home to the general public.

He set up the Science Press Printing Company in 1923 for specializing in the printing of scientific journals and books.

His lifelong interest in problems of university organization and management was strongly tinged with the democratic spirit already mentioned (19). His outspoken views on these problems brought on some of the most exciting episodes of his career and led up to his eventual dismissal from Columbia during the excitement of the first World War. His pacifist leanings and his particular antipathy to any form of compulsion even during war did not prevent him from contributing of his best to the war effort of the psychologists in the development of the Army tests.

Unfortunately Cattell could never be persuaded to write even a brief autobiography. His excuse was that an auto-

biography such as he would write would land him in the position of defendant in a number of libel suits. He felt sure he could not bring himself to delete all the pungent comments that would occur to him, and he had found by long experience that such comments were not always accepted in the spirit of raillery that motivated them in his conversation and in his more polemic writings. Autobiographical material bearing mostly on his early career can be found in some of his writings (22, 23, 24), and considerable material on his life is available in other sources (25, 26, 27).

The crowning honor of Cattell's life came when, at the age of nearly seventy, he was chosen by the votes of American psychologists to represent them as President of the Ninth International Congress of Psychology, held at New Haven in 1929. His presidential address on Psychology in America, with the supplementary materials, makes an important historical document (24).

The present attempt to convey to the younger generation some impression of the life and work of one of our leaders in American psychology may be brought to a close by taking note of the hearty appreciation expressed by his numerous friends. His associates on numerous committees and governing boards speak gratefully of Cattell's broad vision and wise foresight, of his initiative and courage, of his keen sense for effective and yet democratic organization, of his sound judgment of men, of his great power of work and his willingness to give unsparingly of time and thought to the problems confronting an organization, of his ability to integrate the divergent views of a group of men and lead them to a unanimous decision, of his lively wit, and of his warm friendship and personal unselfishness (26).

Visitors to his home, where the latch string seemed to be always out for his colleagues, remember the easy, friendly

atmosphere of that home, with his evident love of children and family life and his delight in the beauty and freedom of the great outdoors.

His old students would certainly be eager to join in a personal tribute to his unfailing interest and generosity. He met the student halfway in the choice of a problem, and while insisting on sincere work by sound methods, he was satisfied with a reasonable achievement. His more promising students were a matter of personal concern to him. He assisted them in many ways, tangible and intangible: guiding them into fellowships and assistantships, supporting their efforts to secure academic positions, and providing employment at scientific work during summer vacations for those who were far from home or in financial need. Of the intangible assistance he gave them, most important was the inspiration that came to the budding young scientists from the kindly interest of one who was clearly a great man and an important figure in the scientific world.

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THE LOGIC OF PREDICTION IN PSYCHOLOGY

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I. INTRODUCTION

To an increasing extent psychologists and other social scientists have become concerned with problems associated with the prediction of behavior. This is evidenced by a number of papers recently published which have dealt with the problem of actuarial versus individual approaches to prediction (14, 30, 32, 36, 37, 42, 46, 53). Several studies have attempted prediction of marital adjustment (11, 47). The Social Science Research Council recently published an extensive monograph which reviewed the literature on the prediction of personal adjustment and pointed the way to some needed research (18). Since this topic has important methodological implications for the psychological and social sciences, it would be appropriate at this time to essay a logical analysis of the concept of prediction.

Probably the most insistent problem has to do with the relative accuracy of clinical or case study predictions versus actuarial or statistical predictions. G. W. Allport, in his brilliant attack on the actuarial method, sets the problem:

Suppose we set out to discover the chances of John Brown to make good on parole, and use for the purpose an index of prediction based upon parole violations and parole successes of men with similar histories. We find that 72% of the men with John's antecedents make good, and many of us conclude that John, therefore, has a 72% chance of making good. There is an obvious error here. The fact that 72% of the men having the same antecedent record as John will make good is merely an actuarial statement. It tells us nothing about John. If we knew John sufficiently well, we might say not that he had a 72% chance of making good, but

that he, as an individual was almost certain to succeed or else to fail. Indeed, if we believe in determinism at all, his chances are either zero or else 100; he is bound to succeed or else to violate because the germs of his future are already contained in his attitudes, in the meaning to him of his antecedent life, and in the specific psychological environment that molds him. Even admitting the possibility of unforeseeable accidents, as scientific determinists we ought to strive for a prediction more accurate than the senseless 72% that is derived from a table of norms based on the antecedents of paroled men en masse. Or again, if seven in ten Americans go to the movies each week, it does not follow that I have seven in ten chances of attending. Only a knowledge of my attitudes, interests, and environmental situation will tell you my chances, and bring your prediction from a 70% actuarial statement to a 100% certain individual prediction (3, pp. 16-17).

Using different terminology, Brown (9) considers two kinds of prediction, a statistical causal analysis and a dynamic causal analysis. Viteles asserts that two kinds of predictions can be made when he writes: "... the statistical point of view must be supplemented by the clinical point of view" (49, p. 134). Bingham and Moore (5) likewise point out the presence of two kinds of prediction: statistical prediction based on regression equations derived from groups, and clinical prediction based upon statistical prediction but refined and modified by including the consideration of factors not treated in the regression equation. Burgess and Cottrell hold a similar point of view. In common with other writers in the social sciences they maintain that because statistical methods deal with averages and probabilities that dynamic

combinations of behavior are excluded. The statistical prediction is considered to be quite limited in its applicability whereas the prediction made from the case study is more valid (11).

These citations are presented merely to reflect the current trends in systematic and applied psychology with regard to the concept of prediction. From these representative excerpts two propositions emerge for further consideration:

- (1) A statistical notion, probability, is involved in the prediction of behavior.
- (2) Two kinds of prediction are made in psychology—the statistical and the clinical.¹

II. PROBABILITY LOGIC IN RELATION TO PREDICTION

The essential notion of probability is understood by nearly everybody and is used in everyday life in all sorts of situations. The popular expressions "I think so," "presumably," "I don't think so," etc., are the ways in which the man-in-the-street uses the concept. But if we stay close to this mythical man-in-the-street we are likely to be led astray. If the same proposition is presented to him and two of his neighbors, he might hold that the proposition was certain to be true, one of his neighbors might declare the proposition to be probable, while the other might express no judgment at all. This might lead us to the erroneous conclusion that the notion of probability is a matter of individual differences. But the probability of the proposition being true is not a matter of personal opinion. It is a matter of logical demonstration.

... The proposition is either proved or not proved, and ... such differences of

¹ The term 'clinical' is used here to express the meaning generic to such terms as 'individual,' 'dynamic causal analysis,' 'case study approach,' etc.

opinion are the result of not understanding the proof. . . . On a given set of data p we say that a proposition q has in relation to these data one and only one probability. If any person assigns a different probability, he is simply wrong. . . . Personal differences in assigning probabilities in everyday life are not due to any ambiguity in the notion of probability itself, but to mental differences between individuals, to the differences in the data available to them, and to differences in the amount of care taken to evaluate the probability (22, p. 10).

Scientists are somewhat more rigorous in their use of the concept of probability than is the man in the street. Instead of using an expression such as "I think so" the scientist considers probability as expressing "a relation between a proposition and a set of data. When the data imply that the proposition is true, the probability is said to amount to certainty; when they imply that it is false, the probability becomes impossibility. All intermediate degrees of probability can arise" (22, p. 9). Pushing one step beyond, we come to a mathematical formulation. If n trials have been made, and if, in these n trials, the event has occurred m times and has failed to occur $(n - m)$ times, then the relative frequency of occurrence is m/n and the relative frequency of failure is $(n - m)/n$. The probability may be said to be "the limit to which the relative frequencies tend when the number of members in the field of statistical measurement approaches infinity" (50, p. 266).²

The concept of probability which hereinafter is referred to as the frequency concept (based on the notion of relative frequency), is rather clearly de-

² It is beyond the scope of this paper to describe or evaluate the various mathematical formulae used in prediction. These are treated in current textbooks. For an up-to-date treatment the reader is referred to the statistical appendices in Horst, Wallin, and Guttman (18), to Hotelling's critique (20), and to Guttman's rejoinder (20).

finer and its mathematics are fairly well-known. No one will question that this frequency concept is at the bottom of the notion of probability used in making statistical predictions.

In order to consider the other form of prediction variously called 'individual prediction,' 'dynamic causal analysis' and 'clinical' prediction, it seems necessary to treat of a second concept of probability. This vague concept does not present itself in a mathematical form. The quotations above taken from Allport, Brown, and others indicate that the frequency interpretation is not appropriate in making predictions of an individual's behavior. Instead of referring to future behavior as 72 chances in 100 of success the clinician would use non-numerical terms, such as 'presumably,' 'probably,' 'more than likely,' etc. This concept cannot be submitted to experimental test. Reichenbach (34) refers to this as the logical concept of probability in contra-distinction to the frequency concept. For clarity we shall refer to it as the non-frequency concept of probability.

Except for a brief controversy between Viteles (49) and Freyd (15) about 18 years ago, psychological theorists and practitioners have considered the problem not at all or as already solved. At that time, Viteles maintained that a statistical clerk could compute for any applicant the statistical probability of success on the job, but that a trained psychologist had to consider other factors which were not included in the regression equation, and "... an adequate diagnosis involves interpretation by a trained psychologist based on the observation of performance and a consideration of related data" (49, p. 134). Freyd countered with a consistently logical argument from which the following is representative:

This (clinical point of view) appears very much as if it were a case of changing horses

in the middle of the stream. The psychologist starts out to measure the abilities required for a job. When he has evaluated statistically a series of tests he states that the tests are inadequate for the purpose and that the applicants can be selected properly only by a supplementary judgment of their performance which the psychologist alone by virtue of his special training is competent to give (15, p. 352).

The clinical method as here characterized is the basis upon which vocational, educational and personnel guidance is administered. First batteries of tests are given, then the clinicians, by supplementary judgments, formulate diagnoses and prognoses.

Freyd states further:

The writer is not in disagreement with the clinical point of view if it means that social, personal history and economic information should receive the same consideration as psychological abilities. These very important facts have been unaccountably neglected by research workers in spite of the fact that in many instances of their use, notably with life insurance salesmen, they have proved to be important. When they are studied, they should be evaluated in the same statistical way as the tests (15, p. 355).

The problem may now be narrowed down to this statement: if the viewpoint expressed by Freyd is correct—that we can apply the frequency interpretation to the non-frequency statement—then it would appear that these superficially different concepts are actually *identical*; if the viewpoint expressed by Viteles is correct, then these concepts are *disparate*. The present author is concerned with this problem because the presence of both interpretations of probability runs counter to the principles of empiricism which demand that predictions be verified. But verification cannot take place without repetition. The frequency of occurrence, then, is seen as a *sine qua non* for prediction. But what of the single case? If an

event occurs but once, how can the frequency interpretation of probability be considered? It is to this question that we turn next.

III. THE SINGLE CASE

Lewin, for one, is opposed to the notion that the frequency of occurrence has any particular significance for the single case.

The accidents of historical processes are not overcome by excluding the changing situations from systematic consideration, but only by taking the fullest account of the individual nature of the concrete case. *It depends upon keeping in mind that general validity of the law and concreteness of the individual case are not antitheses, and that reference to the totality of the concrete whole situation must take the place of reference to the largest possible historical collection of frequent repetitions.* This means methodologically that the importance of a case, and its validity as proof, cannot be evaluated by the frequency of its occurrence . . . (28, pp. 41-2).

With the same insistence upon the individual case is the point of view adopted by Williamson: "Most of our statistical studies are based upon the [fact] . . . that only rarely do we discover an individual whose working conditions even approximate the general conditions of the original investigation. For this reason, the clinical counselor must be extremely careful in applying the results of such studies . . . to predictions for particular individuals. He must modify the *general* prediction in terms of relevant data known about a particular individual" (51, p. 107). The problem before us now is to determine whether we can meet the objection that the single case cannot be dealt with adequately unless we have the two conceptions of probability.

To the clinician the single case represents an individual with a complex of

behavior characteristics which is like no other individual's. The clinician studies this individual, then makes a prediction of the future behavior in terms of probabilities. For example: "Student X has one chance in six of meeting the standards of competition in the University." In keeping with the principles of empiricism, it is necessary to verify the statement of probability. But this is impossible if we deal with only one case. If our student does not meet the standards, we cannot say that our prediction was correct; we cannot tell whether the correct probability-statement should have been $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{10}$ or $1/\text{infinity}$. If we accept as one of the canons of science the verification of predictions, we must deny the single-case interpretation. Up to this point our analysis does not enable us to treat the single event in terms of the frequency interpretation of probability. We have only demonstrated that the clinical or single-case prediction is not subject to verification.

Verification of the probability-statement is possible as soon as the event can be repeated. In other words, as soon as the single case *becomes a member of a class*, the relative frequency of occurrence can be determined and verification of the prediction obtained. The frequency interpretation asserts that the occurrence is not an isolated, individual event, but that it is a member of a class of similar events. The problem now is the construction of the class to which the single case may be ordered.

If the probability statement is to have meaning, the event in question must be considered in the light of similar events. If the clinician can find no similar individuals or events—if the case and the problem are so unique—then he has no experience from which to make a prediction. As soon as he compares the concrete case in front of him with other cases from his clinical experience or from his statistical tables, then he or-

ders the case to a class. According to Reichenbach:

The origin of the single-case interpretation is to be found in the fact that for many cases the construction of the class is not so obviously determined as in the case of the die, or in the fact that ordinary language suppresses a reference to a class, and speaks incorrectly of a single event where a class of events should be considered. If we keep this postulate clearly in mind, we find that the way toward the construction of the corresponding class is indicated in the origin and use of probability statements. Why do we ascribe, say a high probability to the statement that Napoleon had an attack of illness during the battle of Leipsig, and a smaller probability to the statement that Caspar Hauser was the son of a prince? It is because chronicles of different types report these statements: one type is reliable because its statements, in frequent attempts at control, were confirmed; the other is not reliable because attempts at control frequently lead to the refutation of the statement. The transition to the type of chronicle indicates the class of the frequency interpretation; the probability occurring in the statements about Napoleon's disease, or Caspar Hauser's descent, is to be interpreted as concerning a certain class of historical reports and finds its statistical interpretation in the frequency of confirmations encountered within this class . . . (34, pp. 307-8).

This method of verifying historical events is certainly applicable to the other cultural phenomena. Psychologists who maintain that a prognosis is based on data from a single case with no reference to other cases, fail to recognize the frame of reference into which they have fitted their generalizations or else they engage in non-scientific guesswork. Incidentally, the single-case method of interpretation of natural and social phenomena is not new—it has a long history. Primitive peoples utilized single cases to support their beliefs and dogmas. The allegory, the myth, the fable, and so on, are examples of the

single-case used to illustrate or prove propositions. The methodological fallacy, both for the psychologist and the aborigine, is that prediction from the single case takes place without some sort of counting operation. The common-sense inferences from the single case are usually performed with the benefit of informal, subjective statistical comparisons (4).

The operations of those who reject the statistical method of prediction and substitute for it a 'dynamic' clinical or individual prediction, may be described in one of two ways: Either they are making statistical predictions in an informal, subjective, and uncontrolled way, or else they are performing purely verbal manipulations which are unverifiable and akin to magic. Forty years ago Jastrow warned the general public against this practice.

This tendency to insist that the laws of science shall be precisely and in detail applicable to individual experiences possessing a personal interest for us, has wrought much havoc; it has contributed to superstition, fostered pseudo-science and encouraged charlatanism. To antagonize this tendency it is necessary to insist upon the statistical nature of the inquiry . . . (21, pp. 84-5).

The problem can be further clarified by an investigation of the behavioral situations in which we utilize predictions. Why do we make predictions about future events for which we can never have certain knowledge? No one will deny that predictions are practical guides to conduct. Reichenbach puts it this way: ". . . the meaning of probability statements is to be determined in such a way that our behavior in utilizing them for action can be justified" (34, p. 309).

We could follow the behavior of any individual through a typical day to see the role that predictions play in his planning. He mails a letter because there is

a high probability that it will arrive at its destination. He waits for a trolley car because there is a high probability that a car will arrive within the next half-hour. He does not plan a picnic in November because there is a high probability that the weather will not be suitable, and so on *ad infinitum*. Every action falls within a series of actions. If each action is planned in the light of the most probable event occurring, then we are bound to have a large number of successes. If, on the other hand, we do not plan our actions with the idea in mind of the most probable event, then there would be a marked diminution in the number of successes.⁸

To bring into sharper focus the last point, an illustration follows which shows that clinical data which are not submitted to a probability analysis apparently do not improve the accuracy of prediction of academic success. Suppose the clinician is faced with a student who wants to know his chances of succeeding in the University. The usual data, high school marks and college aptitude test scores, provide us with a probability statement that correctly predicts success or failure in seven out of ten cases. But the clinician has more data available than the scores that go into the regression equation. First, this student works 30 hours a week in a restaurant; second, he is in love with a girl back home; third, he does not make a good impression in the interview. On the basis of these three additional observations, the clinician modifies the pre-

diction so that he now tells the student his chances of success are poor, *i.e.*, "Your chances of success are 1 in 10."

His prediction is presumably based on the fact that students in the past who worked 30 hours a week did poorer work than was expected, that love-sick boys did not achieve up to standard, and that students who did not impress him favorably in the interview usually did not succeed. But all available evidence (16) shows that these three variables, when partialled out from the two measurement variables, add nothing to prediction of academic success. If they did, that is to say, if they were associated with successful achievement or failure, then prediction tables could be devised so that the 'clinical' prediction would be unnecessary. If the clinician holds that these three variables apply *only* in this case and not necessarily to others, the burden of proof lies with him. He cannot verify his prediction. If the boy succeeds in school, upsetting the prediction, then the clinician might offer weakly that this case was the one in ten. But if a single case, where are the other nine? If the boy fulfills the prediction, we do not know whether the three designated events had any influence upon achievement, or whether other factors were responsible. The prediction can be verified only by ordering the event to a class and then counting the number of successes. It matters not whether the class is intra-individual—determined from events occurring in the individual's reaction biography, or determined from events occurring in a population of individuals.

In the foregoing paragraphs I have followed Reichenbach in attempting to demonstrate that for scientific prediction the two concepts of probability are essentially the same and that the frequency interpretation stands unassailed. In fine, both concepts rely upon ordering to a class of events, any particular

⁸ Bridgman makes some pithy comments in regard to the application of the mathematical solutions in individual events. "... people seem unwilling to recognize that probability roughly measures their confidence in their plans for future action, but they seem to demand something sharper. Everyone recognizes that if he has to devise unique plans for the future he is a fool if he does not base his plans on the 'most probable' eventuality" (7, p. 99).

event for which we wish to make a probability statement or prediction. The class may be defined as events of a similar nature which occur for groups of people, or events of a similar nature which occur at different times to the same individual. "We need not introduce a 'single-case meaning' of the probability statement; a 'class meaning' is sufficient because it suffices to justify the application of probability statements to actions concerned with single events" (34, p. 312).

IV. CONCEPT OF THE CLASS

The concept of the class needs further clarification. As usually interpreted, and as used in the preceding section, the class means a grouping of events. Suppose, however, we were interested in an individual who was in a class by himself. Suppose he were a Harvard professor and we wanted to predict whether he would attend the movies on any particular day. In the absence of information on our professor we would make a prediction on the basis of the movie-going habits of college professors rather than people-in-general. The class, then, becomes the group of college professors. But in our supposition we are ignorant of the habits of this group. Therefore, we study the movie-going habits of our professor over a period of time independent of others. The class now consists of events—'attendance at movies.' Our study might reveal that the professor attends the local theaters on the average of once a month. Therefore, for any single day we would make the prediction that the chances are $\frac{29}{30}$ for non-attendance. The most probable event is that he will not attend on any particular day. We will have the greatest number of successes if we predict that he will not go on any given day. Over the period of a year, we may be wrong 12 times; but we shall be correct 353 times. From this illustration it

would appear that actuarial predictions are appropriate for individuals as well as for groups.⁴

V. CLINICAL AND STATISTICAL METHODS OF PREDICTION COMPARED

The statistical method of prediction is essentially the same for all types of criteria. Whether predicting the performance of an individual operating a welding-torch, the efficiency of a certain drug in combatting a disease process, success in college, or adjustment in marriage, the same general methodology prevails. Predictions are made on the basis of empirical observations.

The steps in the method may be summarized as follows: (1) An explicit statement of the criterion to be predicted is formulated. (2) Variables or attributes presumably associated with the attainment of the criterion are estimated. These may be psychometric scores, items from a reactional biography, physiological signs, or social facts such as marital status, urban or rural residence, etc. (3) These variables or attributes are correlated with the criterion variable. The most simple case is illustrated below with a four-fold sur-

N = 1200	Percent improved after hospitalization	
	Percent improved after hospitalization	Percent unimproved after hospitalization
Presence of shut-in personality in childhood	12	88
Absence of shut-in personality in childhood	59	41

face.⁵ (4) Wider ranges of measurement for criterion and predictor vari-

⁴ The 'teleonomic' conception of studying personality considers predictions made for one individual on the basis of multiple observations of a single case. See F. H. Allport (1).

⁵ Data from Sarbin and Wittman (37).

ables call for more complicated procedures. The underlying notion, however, is the same. (5) Weights are assigned according to observed differences. (In the illustration above, a weight of 47 is assigned to the presence of the item in future cases, 0 to absence of item.) (6) Where there are multiple predictors, the multiple regression equation can be used to determine the contribution of each variable to the total prediction score. (7) The probability-value of the prediction made from the regression equation is available prior to the forecast. (8) Finally, from the equation, or from tables or nomograms derived from such equations, predictions for a new set of individuals are made with known margin of error.

The clinical method offers a sharp contrast to this rather uninspired, mechanical method. Although both methods call for a prior statement of the criterion or predictand, the clinical or intuitive worker is not bound to any systematic methodology in arriving at his forecasts. He may begin with the same data that are put into the regression equations, or may actually use such equations. But, if he wishes, he can disregard, modify, or magnify any measurement on the basis of his previous experience or clinical intuitions. He may have a kind of informal statistical method (of which he may be unaware [36]) but he is not bound by any rigorous mathematical procedures. His methods are the very essence of flexibility and he is free to apply whatever meaning (predictive-value) he chooses to data about an individual. Further, and this is considered of utmost importance by the protagonists of the non-statistical method, the clinician usually has much more information at hand about any one person than can be conveniently used statistically.

Let us examine this last proposition that clinicians usually have more data

available than go into the regression equation. The obvious and plausible assumption is that the more data, the more accurate the prediction. In prediction of psychological criteria, the clinician will use ratings, paper-and-pencil tests, health reports, school histories, inventories, projective personality tests, and interview-data. Although the clinician does not have a regression equation in front of him, he may treat the data as though he had. In so doing, he is using an informal, implicit, statistical method. There is nothing super-empirical about it. In the clinical situation, the clinician's implicit regression equation may have 15 or 20 or 50 variables. The clinician may apply the same weights systematically to all cases, or he may apply the weights differentially in each individual case. The clinician, in other words, makes judgments on the basis of a large number of facts taken from different sources. Because predictions are usually made in a relatively brief period of time, it is impossible for the clinician to perform all the mathematical operations necessary. Thorndike, writing in 1918, analyzes what happens when a so-called intuitive judgment is made.

The competent impressionistic judge of men does respond to these interrelations of the facts and sums up in his estimate a consideration of each in the light of the others. If there are ten traits involved, say ten entries on an application blank, he may be said to determine his prophecy by at least $10 + 9 + 8 + 7 + 6 + 5 + 4 + 3 + 2 + 1$ quantities, since he responds to each trait in relation to all the others. There is a prevalent myth that the expert judge of men succeeds by some mystery of divination. Of course, this is nonsense. He succeeds because he makes smaller errors in the facts or in the way he weights them (48, pp. 75-76).

Elsewhere (36) the author has given a free-association account of a typical

clinical prediction. Space limitations prohibit its reproduction here. Suffice it to say that the introspective account indicates that clinical predictions are made on the basis of (1) deductions from known and tested generalizations (informal or implicit statistical predictions); (2) deductions from plausible but untested hypotheses; (3) deductions from false generalizations; and (4) so-called intuitions.

With regard to the first item above (1) it is apparent to anyone that deductions from tested generalizations can be accomplished with greater precision and efficiency by using prediction tables or equations. In regard to (2), deductions from untested hypotheses, a clinician may find himself embarrassed when the superficial plausibility of his hypothesis is exploded by empirical or experimental studies (36).⁶ As to (3), it is needless to mention that clinical predictions are not made more acceptable by the use of deductions from false generalizations. (Parenthetically, these are more common than most clinicians realize.) The point where the clinical method seems to be different is in (4), the use of so-called intuitive data which have not and cannot be subjected to the same scientific tests as those which are amenable to empirical observations.

The shift in American psychology in recent years toward concepts variously named *Verstehen*, intuition, sympathetic understanding and insight has caused us to stop and question whether this method of comprehending data will replace the more conventional form or will be a supplement to the logical type of understanding that we have traditionally used.

In order for us better to understand what is meant by the intuitive method in psychology as opposed to the empirical method, we must consider the differentiae between the natural sciences, and

the cultural sciences. Natural sciences are said to be nomothetic, that is to say, they seek laws, causality, etc. Cultural sciences, on the other hand, seek uniqueness, individuality, they are idiographic. So far the differentiae do not lead to any schism in psychology. Uniqueness or individuality can be studied with the same empirical methods as behavior common to groups (2).

The differentiae lie elsewhere, namely in the conception of psychology as a science. Klüver has remarked "Whether there is one scientific psychology or whether there are two kinds of psychology, whether there is a necessary antagonism between 'causal' and 'understanding' psychology, or whether they supplement each other—these and kindred questions have not been answered with finality" (27, p. 455). A brief acquaintance with the writings of those who profess to follow the psychology of understanding will immediately raise the question: Is this kind of psychology a science?

In this connection, Klein has written a paper describing the nomothetic and idiographic concepts. He points out that there are four ways of understanding things: (1) a structural continuity type, (2) a functional type, (3) a logical or implicative type, and (4) an empathetic type. The first is illustrated as that which uses mechanical models. The second deals with concepts, such as those of physics and chemistry, e.g., gravitation. The third is the method common to all science—the inductive method. The fourth method of understanding, empathy, is

The task of fathoming human motives or appreciating the entire gamut of human desires. To understand the football player struggling for a touchdown, the philologist tracking down an obscure root, . . . requires neither mechanical models, a knowledge of functional relationships, nor expert mastery of local implications. But it does require a knowledge of human na-

⁶ Also see p. 34 f. of this article.

ture. It represents the type of understanding indispensable for the development of psychology as a social science or as a *Geisteswissenschaft* (26, p. 565).

The first three methods, he goes on to say, have a place in psychology, but the idiographic method, characterized by the above quotation, will lead to the most fruitful generalizations. One question that is not answered is this: how does one acquire "a knowledge of human nature"? Windelband has an answer:

The psychology which the historian uses is a very different thing [from scientific psychology]. It is the psychology of daily life: the practical psychology of the poet, and the great statesman—the psychology that cannot be taught and learned, but is a gift of intuitive intelligence, and in its highest form a genius for judging contemporary life and posterity. This sort of psychology is an art, not a science (52, p. 280).

The writer has no space to repeat the logical argument presented in another paper (40) which rejected artistic, intuitive methods in psychological practice and research. Talking about such super-empirical vagaries as the 'gift of intuitive intelligence' is little short of autistic conduct. To posit, as G. W. Allport does, "a structuring activity of the mind" (2, p. 547) seems gratuitous to any sophisticated psychological system.⁷

⁷ Sophisticated, that is, as of 1943. Sophistication in scientific constructs calls for formulations that embrace the present orientation in science. Kantor points to five trends which illustrate this newer approach: (1) relativity theory, (2) quantum mechanics, (3) the indeterminacy principle, (4) "the Kant-Bridgman discrimination against meaningless questions, the consequence of which is that only such problems are considered valid as can be solved by observation and experiment—in short, complete operations of the scientist. To these must be added (5) an equally cogent consideration—namely, scientific success is a definite function of the freedom from conventional bias with which the scientist approaches his field of operation" (25, p. 175).

R. B. Cattell (13) also offers a logical argument to show the illusory nature of the intuitive approach when applied to psychological formulations.

The intuitionist desires to know another personality directly, by empathy instead of in an objective fashion. . . . Because the mind of another human being is more akin to his own than is the mind of an insect, he is misled into believing that he can apprehend it directly. . . . It is the especial danger of psychology that the psychologist is encouraged to use intuition because by reason of the partial similarity of minds, it yields an attractive proportion of correct guesses in predicting the thoughts, feelings, and behavior of others. But even in practice its use leads in the end to errors more impressive than its successes. . . . Every intuitive judgment made in a psychological clinic is a . . . stereotype distortion of a unique mind. Intuition has an indispensable place in research, as a scaffolding under the shadow of which objective investigations may be built up; but propounded as an independent method of arriving at psychological knowledge, it would seem to be a pure illusion (13, p. 130-131).

The previous paragraphs may appear to contain an obvious bias against intuitive methods of personality study. The intuitionists—who usually get along so well without empirical data—by this time must be clamoring for evidence. All these polemics, they may assert, mean nothing in the absence of evidence which would disprove the hypothesis that predictions based on intuitions are more accurate than predictions made from the less exciting equations of the actuarial method. Therefore, we turn to the available evidence.

VI. EMPIRICAL STUDIES

The empirical studies that have been reported to date seem to favor the actuarial method. They are briefly reviewed below.

(1) Wittman (53, 54) has offered

evidence on prognosis in schizophrenia. Predictions of outcome of hospitalization were made by the psychiatric staff on the basis of anamnesis, mental examination, physical examination, conference with patient, and discussion of the case in a diagnostic staff conference. These clinical predictions were made on an intuitive, i.e., non-statistical basis. Statistical predictions were made with the aid of a 30-item rating scale which assigned weights of items appearing in the anamnesis or mental examination.⁸ Her results showed the unmistakable superiority of the scale over the staff judgments. Table 1 shows these differences.⁹

TABLE 1

PERCENTAGE OF CORRECT PREDICTIONS OF THE RESULT OF HOSPITALIZATION WITH PROGNOSIS SCALE AND WITH MEDICAL STAFF PROGNOSTIC JUDGMENTS

	Prognosis Scale	Medical Staff
Remission.....	90	52
N = 56		
Much Improved.....	86	41
N = 66		
Improved.....	75	36
N = 51		
Slight Improvement...	46	34
N = 31		
Unimproved.....	85	49
N = 139		

(2) A continuation of this study was carried on by Sarbin and Wittman (37). In the former study, the scale items were arbitrarily though systematically weighted according to the author's experience with thousands of dementia precox patients. This system of weighting allowed for a certain degree of subjectivity and incommunicability. In the continuation study, therefore, the weights were derived empirically. The scale was

⁸ The reader will note that case study material is possible of quantification and statistical treatment in the same manner as other kinds of data, such as I.Q.'s. The terms 'statistical' and 'psychometric' are not synonymous.

⁹ Adapted from Wittman (53).

reduced to 22 items. On a new sample of 200 patients, the comparisons were similar to those of Table 1.

(3) A third study was reported by the present writer in which clinical predictions of academic achievement were compared with predictions made from regression equations. The clinical predictions were presumably made on the basis of the multifarious data available to the clinician. These included numerous tests of aptitude, achievement, personality and interest; a personal record form; the written observations of a preliminary interviewer; and whatever information the clinician could gather in the face-to-face interview. The results are reported in Table 2.¹⁰ The com-

TABLE 2

COMPARISON OF CLINICAL AND STATISTICAL PREDICTIONS OF ACADEMIC SUCCESS AND FAILURE

Achievement	Clinical Predictions	
	Success	Failure
Success	75	35
Failure	13	39
Statistical Predictions		
Success	75	26
Failure	13	48

$$\chi^2 = 5.63; P < .02$$

parisons seem to favor the statistical method of prediction. From this study we can say with some assurance that the prediction of academic success or failure can be done simpler and with greater accuracy by the statistical method.

(4) Burgess (21) cites a study by Scheidt (42) on the prediction of parole violation. The evidence in the prediction of this criterion seems to corroborate the other empirical studies. The mechanical predictions were superior to those made by the prison physician.

The preceding argument together with the empirical studies just cited would seem to throw considerable doubt on the

¹⁰ Data from Sarbin (36, 38); statistical treatment according to Snedecor (41).

validity of non-inferential intuitive modes of predicting conduct.¹¹

VII. PREDICTION AND THE APPROXIMATE CHARACTER OF KNOWLEDGE

The view expressed by some writers that predictions must be stated as one or zero likewise needs examination. G. W. Allport, for one, declares that a thorough knowledge of one's interests, attitudes, and environmental situation will raise an actuarial prediction of 70 per cent (in his illustration) to '100 per cent certain individual prediction' (3, p. 17). The case study (2) is suggested to point the way to this scientific El Dorado.

Illustrative of this same view is Brown's statement, to which a reference has been made previously. He asserts that 'dynamical causal analyses' express predictions in terms of *exact* amounts (9). The implications of those who speak for the superiority of the clinical method as opposed to the more approximate statistical method force us to think in terms of probability statements of one or zero. The same notion obtrudes from this statement by Burgess and Cottrell:

... while statistical prediction may be valid for averages and for stated probabilities that results will fall within certain defined limits, *it can only be approximate.*

Implied in the above-noted limitations is the fact that present statistical methods deal with averages and probabilities and not with specific dynamic combinations of factors . . . (11, p. 33) (*italics mine*).

¹¹ Polansky's study (31) has sometimes been cited as evidence that clinical methods produce better predictions than statistical. This is an error in that it has been assumed that statistical predictions are always made from psychometric data. Polansky showed that for his three cases, predictions made from data presented in 'structural analysis' forms were more accurate than predictions made from other forms of analysis—including psychometric. The problem of intuitive, clinical prediction versus actuarial was not actually considered by Polansky.

In the absence of objective data on larger numbers of cases, these authors then go on to place their faith in 'case studies' for the important configurations of dynamic factors (11, p. 340). From these statements, the reader of their monograph infers that the case study method yields predictions which are based upon 'specific, dynamic combinations of factors' and which are not approximate, but exact.

The point which is missed, probably because it is taken for granted, is that the results of all measurement are only approximate. Psychologists, in using tests and measures of the traditional sort, expect only approximate results. Usually however, this expectation is based on the assumption that the results are approximate only because the test or measure is a psychological one, not a physical one. But all measurement, regardless of the object to be measured or the scientific label attached to the individual who is taking the measurement, is approximate. To quote Bridgman:

That such is true [the approximation of measurement] is evident after the most superficial examination of any measuring process; any statement about numerical relations between measured quantities must always be subject to the qualification that the relation is valid only within limits. Furthermore, all experience seems to be of this character; we never have perfectly clean-cut knowledge of anything, but all our experience is surrounded by a twilight zone, a penumbra of uncertainty, into which we have not yet penetrated. This penumbra is as truly an unexplored region as any other region beyond experiment, such as the region of high velocities, for example, and we must hold no preconceived notions as to what will be found within that region. The penumbra is to be penetrated by improving the accuracy of measurement . . . (8, pp. 32-34).

It is not by intention to belittle the contributions that have been made and that will be made by the case study ap-

proach; I merely want to caution against the prevailing tendency to think of this method as the only salient against the approximations we are forced to make because of the very nature of measurement. This caution is reflected in the discussion of a more predictable criterion of academic achievement by Sarbin and Bordin (39). They have accepted this axiom and question whether predictive coefficient even of .95 will ever be attained. A reference to Heisenberg's principle of indeterminacy, which has proven so fruitful in modern physics, supports their conclusion that correlation coefficients between psychological measurements will never attain a point where predictions of one or zero can be made.

A critic may suggest that the experiments of physical theorists cannot be applied to the problem of measurement and prediction in psychology. He may even warn us against reproducing the errors of an earlier psychology which uncritically adopted the concepts of the physical sciences and which tragically turned out to be sterile. Heisenberg's indeterminacy principle, however, is a general statement for all science. It is based upon the experiments in modern physics in which attempts were made to measure the simultaneous positions and velocities of electrons. In general, it was discovered that when an attempt is made to measure velocity, the position is disturbed; when the position is measured, the velocity is disturbed. That we have analogous and even similar situations in psychological measurement will be revealed below. At this point it would be informative to quote Heisenberg's introduction to his discussion of the indeterminacy relationship.

Particularly characteristic . . . is the interaction between observer and object; in classical physical theories it has always been assumed either that this interaction is negligibly small, or else that its effects can be eliminated from the result from calcula-

tions based on 'control' experiments. This assumption is not permissible in atomic physics; the interaction between observer and object causes uncontrollable and large changes in the system being observed, because of the discontinuous characteristic of atomic processes. The immediate consequence of this circumstance is that in general every experiment performed to determine some numerical quantity renders the knowledge of others illusory, since the uncontrollable perturbation of the observed system alters the values of previously determined quantities. If this perturbation is followed in its quantitative details, it appears that in many cases it is impossible to obtain an exact determination of the simultaneous values of two variables, but rather that there is a lower limit to the accuracy with which they can be known (17, p 3).

The indeterminacy relationship is admirably suited to psychological measurement. Every attempt at measurement introduces changes in the event which precludes our obtaining an exact representation of that event. The interaction between observer and object is as much a reality in psychology as in physics. Any prediction made from obtained measurements must therefore allow for a margin of error—the prediction must be stated as an approximation rather than as an exact value. This holds for macroscopic science as well as for atomic physics. Three illustrations of this principle of indeterminacy as applied to psychological measurement are here presented.

A. Taking an experiment at the psychophysiological level, suppose we wish to determine blood pressure while the individual is being presented a list of stimulus words. The very act of applying the cuff of the sphygmomanometer introduces changes in the blood pressure which are not and cannot be measured. Other changes in the event are brought about by the measurement process—these 'uncontrollable perturbations' belonging more particularly to the whole psychological field than to the initial act of applying the cuff. The pres-

ence of the observer will alter some of the characteristics of the field. The unfamiliarity of the subject with the laboratory situation likewise may result in changes in the blood pressure which are not controllable. The sphygmomanometer reading must therefore be regarded as an approximation.

B. Suppose we wish to assist a student to discover whether or not he should become an engineer. He makes a verbal statement, "I want to become an engineer." We begin to make measurements. In this case we use paper-and-pencil tests. Suppose we use, among other measurement devices, a placement test in mathematics. When the student subsequently reports for an interview, his verbal statement has been changed to "I want to become a business man." The act of taking a measurement of mathematics achievement served to change the verbally expressed interest statement. The changes to the organism being measured by paper-and-pencil tests are usually not as obvious as in this illustration. Experience with hundreds of students who have been subjected to psychological tests of all kinds has convinced the writer that such changes do occur. Of course, these perturbations may not have great significance in terms of planning for future actions. The proposition is submitted, however, that the act of taking a measurement—even with a paper-and-pencil test—introduces certain alterations in the psychological field which precludes our making an exact measurement.

C. Obtaining measurements by means of an instrument such as a sphygmomanometer or of a test such as a placement examination in mathematics show certain resemblances to the situation in the physical experiment described by Heisenberg. Application to the psychological interview is less obvious. The interview is not designed to take measurements as we ordinarily think of them. The interviewer usually determines the presence or absence of certain characteristics in the reactional biography of the interviewee. In some cases he applies values to degrees of presence or absence of traits. That is to say, he makes ratings. The gathering of information by the interview method is an analogue to the

taking of measurements in other disciplines. The interviewer, or perhaps the total interview situation, is to be regarded as the measuring rod. In either case the data that are elicited from the interviewee are conditioned by the total field of which he is but a part. When the clinician enters into the total psychological field of a patient, he cannot avoid a train of consequences which may work to the advantage as well as to the disadvantage of the patient (45). Such known mechanisms as the identification of the clinician with, say, a domineering parent; the kind of question asked; the strangeness of the social psychological situation; these and many other factors influence the accuracy of obtaining information in the interview.

The foregoing illustrations are intended to support *a fortiori* the proposition that predictions of 1 or zero cannot be obtained because of the approximate character of measurement, and further, that no form of measurement, be it the measurement of intelligence by tests or of certain 'intangible' factors by the clinical interview, can be stated with certitude. As Bridgman has stated: "It is a consequence of the approximate character of all measurement that no empirical science can make exact statements" (8, p. 34).

The writer will not quarrel with a critic who replies that it is too premature in the development of our science to invoke the Heisenberg indeterminacy principle. Certainly the elimination of errors in observing, recording, and interpreting human conduct would greatly increase our predictive powers. So would the abolition of the autistic search for old demons in new garments, for instincts with new names, etc. (23). This discussion is offered principally to show the untenability of such statements as "Indeed, if we believe in determinism at all, his chances are either zero or else 100 . . ." (3, p. 17) rather than to pass off our imperfect predictions behind the cloak of the Heisen-

berg principle of indeterminacy. The scientist must evermore strive to penetrate the penumbra of uncertainty surrounding every conclusion. Boring's remarks are apposite here:

With the Heisenberg principle itself one cannot quarrel: When we must be ignorant, we must be ignorant. Let us not encourage ourselves, however, into any patient acceptance of any ignorance that can be dispelled. The use of statistical indeterminacy in most macroscopic science is an unnecessary and premature denial of the effectiveness of determinism (6, p. 301).

In line with this discussion, we might point out that some social scientists deny that predictive generalizations can be made at all. They hold that psychological, social, and cultural events are so unique and so complex that predictions from generalization of observations are beyond the realm of possibility. In short, as Kimball Young (55) has contended, the future is *indeterminate* for personal or social events in contradistinction to natural events which recur and are therefore predictable.

Sorokin and Berger (44) also make this point. After showing that individuals can predict accurately four-fifths of their activities for the next 24 hours, they go on to theorize on the possibility of predicting social events. They maintain that most predictions of social events are little more than guesses and should not be paraded as science. The difficulty here is that social scientists have not succeeded in formulating their problems so that apparently unique events may be ordered to a class. These writers are too pessimistic. Lundberg is more optimistic for the future of social science.

The apparent unpredictability of group behavior . . . is due to our present limited knowledge of the nature of stimuli and responses operative in such groups. Some of the simpler chemical reactions were considered unpredictable some centuries ago

for the same reason. Painstaking observations and classification of these reactions under given conditions, however, yielded our present chemical laws. The same method has already yielded us some power of generalization and prediction of group behavior. There is every reason to believe that we can vastly extend this power (29, p. 22).

Advances in modern scientific methodology may suggest ways of treating their complex data so that predictions—utilizable for action—may be stated. The shibboleth of *complexity* of subject matter so often stands in the way of scientific progress in the social and psychological sciences. Despairing social scientists look longingly at the greener pastures of the physical sciences where events are considered to be less complex. In this connection, the statement of Sir Ernest Rutherford, President of the British Association for the Advancement of Science (in 1923) is enlightening:

When we consider the extraordinary complexity of the electronic system, we may be surprised that it has been possible to find any order in the apparent medley of motions.¹²

VIII. INTERPRETING CLINICAL HYPOTHESES IN TERMS OF PROBABILITY

The statistical method of prediction, it is submitted, is fundamental to diagnosis and to the prescription or choice of treatment in clinical psychology and psychiatry. The discussion so far takes from the clinician many of his functions and assigns them to the psychologist in the laboratory. One major aspect remains—one that is seldom recognized by practicing clinicians—namely, *the formulation of hypotheses*. The present author maintains that this is the most significant function that the clinician can perform. Every case abounds in hy-

¹² In Buckley (10) quoted by Lundberg (29, p. 10 n.).

potheses, many tested, many waiting upon the research psychologist for testing. No other area of psychology is so fertile in the production of hypotheses. Every time the clinician has a 'hunch' he formulates an hypothesis. In the absence of crucial experiments these hypotheses are often treated as 'laws.' If predictions from the hypothesis are verified, then the hypothesis may be stated as a principle.¹³

This series of events is idealistic, to be sure. In practice, many of our hypotheses are left untested. The clinician many times is forced by the demand for action to utilize the untested hypothesis as though it were tenable upon evidence. For example, many student personnel workers have counseled students from the hypothesis that agreement of a student's educational-vocational choice with ratings on the Strong Vocational Interest Blank contributes to academic achievement. The hypothesis is plausible. Segel and Brindle (43), however, have demonstrated that this instrument, as conventionally used, adds practically nothing to the prediction of academic success. Another hypothesis frequently transformed into principle relates in universal manner academic achievement and outside work. Reader and Newman (33) have shown that the hypothesis is untenable.

This treatment of hypothesis as verified principle is an example of what has been referred to in another paper as one of the meanings of clinical psychology as *art*, viz., 'individual explorations into the unknown' (40).

In his chapter 'The Art of Diagnosing'

¹³ In a more formal sense, the clinician is interbehaving with phenomena. This is one of the steps in Brown's exposition of the hypothetico-deductive method. He minimizes, however, the importance of the behavior which leads to the 'hunches.' See J. F. Brown (9) for an exposition of the hypothetico-deductive method. For a critique of this methodology, see J. R. Kantor (24).

Williamson (51) not only condones but encourages using hypotheses as yet unverified by research.

Some personnel workers assert that diagnosing must be scientific, that the research or experimental approach is the only valid one. While personnel research is necessary for effective analysis, yet diagnosing is not scientific. Although the counselor must use facts, principles and generalizations derived from experimentation, he must also use hunches (insights, reduced clues, intuitions) and hypotheses unverified as yet by personnel research. Moreover, while indebted to statisticians for valid generalizations derived from analyses of groups, the counselor must be constantly alert in inferring whether these generalizations are *validly applicable* to the particular student being counseled. If such a student does not possess characteristics similar to the group from which the generalization was derived, then it would be a distortion of logic, as well as of science, to diagnose this student on the basis of the group generalization (51, p. 105).

This statement is based on a sort of rational belief and is at variance with empirical findings cited above and with the fundamental propositions reiterated before, viz., in the absence of knowledge of the predictive powers of single-appearing data, the probability based on *observed* relative frequencies is taken as the most accurate prediction.

We must return for a moment to the theory of probability for an understanding of this use of hypothesis as fact. The probability of occurrence of an event is expressed as the limit to which the relative frequencies tend when the number of members in the field of statistical measurement approaches infinity. It was agreed that the scores or class to which we ordered our data might appear too general, too heterogeneous, to formulate a meaningful probability-statement for the single case. To this point Von Wright has written:

... if there are special circumstances which make the rational degree of belief in an event (q) to differ from the limiting frequency of the event in such series (p), it is because if there were a probability series, defined by these special circumstances, then the limiting-frequency of the event in question in this series would be that value q . And this hypothetical statement is just what makes the degree of belief q in the event, under these special circumstances *rational* (50, p. 283).

When a clinician predicts behavior on the basis of a plausible but untested hypothesis (one in which he has a 'rational degree of belief') he unwittingly postulates a class or series of which the event in question is a member. Else how would he be able to attach a value to the event? The writer has read case records in which one clinician regarded the influence of 30 hours per week of outside work on the academic achievement of a high aptitude student as deleterious, while another clinician regarded the same event as beneficial because it was a sign of strong motivation.

The inference must not be drawn that clinicians should cease making hypotheses. The recognition of this hypothesizing function is a step in the direction of making clinical psychology more scientific and less artistic (35). The clinician's hypotheses may contribute significantly not only to applied psychology, but also to theoretical science. His hypotheses arise out of interbehaving with concrete human beings in specific social psychological situations. The lack of significant progress in many branches of psychology might be remedied by placing theoretical psychologists in close touch with significant human problems which come to light in the clinical situation.

A brief digression might clarify the meaning of the last statement. The clinical psychologist too often preoccupies himself with phenotypic descrip-

tions. That is to say, his daily contacts with patients, clients, or counselees are expressed in 'protocol language,' in 'statements belonging to the primitive protocol' (12). The theoretical psychologist, on the other hand, tends to concern himself primarily with genotypic descriptions. He is interested more in the general laws of nature, in the statements of general propositions from which singular propositions can be derived.

Greater cooperation between the clinical and the theoretical psychologists will result in more meaningful research and theory. The designs for experiment based on clinical hypotheses may lack the simplicity and perhaps the rigor of the animal laboratory, but the results will be more meaningful as guides to human action if we accept the principle behind the statement of Hogben that "the real credentials of a science lie in its capacity to yield information which is a guide to practical conduct" (19, p. 189).

IX. SUMMARY

The prevailing conceptions with regard to prediction of behavior have been outlined. There seem to be two schools of thought as to the nature of prediction—so-called intuitive methods and inferential methods. The postulation of two forms of prediction necessitated the further postulation of two interpretations of probability. Empirical data and logical argument have been submitted in evidence of the fact that the two interpretations of probability are essentially one, namely, the frequency interpretation. All meaningful prediction is based on informal or formal statistical (inferential) manipulations.

An inference drawn from the writings of those who hold that predictions are based on other than statistical data is this: Statistical predictions are only approximative—clinical or case-study predictions are exact. In order to refute

this statement, certain propositions were offered, among them applications of the Heisenberg indeterminacy principle which illustrated the scientific principle that all knowledge is approximate.

The use of unverified hypotheses by trained clinical workers is considered unwarranted. This does not mean that clinicians should cease hypothesizing. Hypotheses are the essential ingredients for building up not only diagnostic and treatment procedures, but important psychological theory as well. Every case contains many hypotheses. These the clinician should turn over to laboratory and field workers to test, or he should turn experimenter himself and set out to see if predictions made from his hypotheses can be verified. When the clinician uses an untested hypothesis as fact, he wittingly or unwittingly places a probability value upon it. The accuracy of this value can never be known unless subjected to test.

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GERMAN PSYCHOLOGY UNDER THE NAZI SYSTEM: 1933-1940¹

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1. *Introduction.*—The development of German Psychology since the beginning of the NS² dictatorship in 1933 should be judged not only from the point of view of changes brought about by an absolutist political system based on an irrationalist ideology, but also from the points of view both of certain traditional attitudes held in German psychology and of developmental forces already active for some time prior to 1933. Psychology in Germany was a field of high accomplishment, and of an elaborate institutional character, based on strong traditions. Psychology as a systematic science had its origin in Germany, and up to the first decade of the 20th century German universities were recognized as the creative centers for this relatively young science. The events of 1933 found various 'schools' of certain famous psychologists³ connected with schools of thought of a more general (philosophical) nature which, in turn, were the expression of still deeper and more universal cultural trends.

* At present U.S.N.R.

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² NS stands for National-Socialism, or National-Socialists, or National-Socialistic.

³ The establishment and existence of 'schools' of psychology usually representing the ideas of the head of a department of psychology is a tradition much more fully established in Germany than in the United States. To recognize the regius professor of a department also as the head of an ideological school, as well as to feel oneself as belonging for a time to such a 'school' is, for the average German scholar, simply a matter of fact. Nor is this phenomenon without significance: it is related to the character of many German professors of psychology, as aspiring prophets and masters.

Some of these trends in philosophical thought (108) were not without ideological kinship with NS, or at least could be made appear to be so through more or less subtle shifts in emphasis and definitions. Under the impact of the new System, which rapidly succeeded in taking control of all forms of practical and intellectual life, such trends quickly were brought to the fore, greatly enhanced and, to a certain degree, unified. At the same time a number of the leading psychologists were dismissed from their positions for racial or political reasons, irrespective of their scientific *Weltanschauung*. Most of them emigrated and have succeeded since in carrying on their work in other countries, while their disciples have endeavored to continue essentially along the old lines. Since in most cases these disciples did not dare or want to give the names of their former teachers, such work is frequently presented without reference to the origin of its most basic principles (19, 21, 43, 56, 113).⁴

A few heads of departments were avowed followers of the Nazi Party and ideology long before 1933, notably E. R. Jaensch in Marburg and F. Krueger in Leipzig. The majority of Germans connected with academic institutions had always been oriented toward a less ar-

⁴ According to the most recent computation available, twenty-seven psychologists, who once held academic positions in Germany had by 1936 been dismissed—according to *Notgemeinschaft Deutscher Wissenschaftler im Ausland* (London, Autumn, 1936). Subsequent years have swollen this number considerably. Prominent German psychologists now in America are Kurt Goldstein, Wolfgang Köhler, Kurt Lewin, Erwin Straus, Max Wertheimer, Heinz Werner.

ticulate romantic conservatism characteristic of certain German academic quarters. They were thus, from the start, favorably inclined towards nationalist and militaristic tendencies. Probably the largest number of psychologists readily submitted, as in all other social fields, post factum, to the new regime and its ideology once it had been established and had made it clear enough that it was there to stay. Sensing the opportunity for extraordinary advancement and for power and fame, they quickly made the best of the situation. Their conversions found foremost expression in the hurried introduction of the principal tenets of NS into psychological thought. Nevertheless, since 1933 there have been published in psychological journals a large number of papers, which bear no marks of the new spirit. Especially is this the case in the more impersonal, objective fields of psychology. Conversely, the more philosophical a particular research, the more marked was the impact of NS ideology.

2. *Influence of Philosophy.*—The influence of philosophy on psychology has always been greater and more direct and the relations between the fields much closer in Germany than in this country. In Germany many psychologists published philosophical books and the psychologist's *Weltanschauung* usually found decisive and articulate expression in the fundamentals of his research. In order to understand the currents in German psychology it is necessary to consider for a moment the philosophical premises on which they are built. Two trends of philosophical thought especially have influenced recent German psychology (9).

One line of development, academic in character, begins with the phenomenological movement which exercised its influence on psychology in three different stages: (1) There was the formalism of Husserl's phenomenology which, especially through the versatility of Max

Scheler from the twenties onward, attained an ever-increasing influence on psychological methods of description, e.g., 'levels' of personality, direct comprehension of structures, etc. (57, 58, 138, 139, 140). (2) The transcendentalism of the later Husserl may be said to anticipate the organismic-holistic attitude toward personality and thus to encourage the description of personality as something intrinsically different from 'things' (latent attitude of anti-physicalism, 59). (3) This latter tendency has become the main principle of *Existenz Philosophie* as set forth by the philosopher Martin Heidegger and the psychiatrist Karl Jaspers (49, 50, 51, 52, 72, 73, 74). Thus the conceptual realism originally held by phenomenology turns at last into an extreme subjectivism. The individual is thought of as determining himself, as creating his real self in the act of vital decision. *Decision* in this sense is thus absolutely primary in an irrational world where real *insight* is impossible.

The irrationalism of *Existenz Philosophie* is akin to the second main trend of influence, the *Lebensphilosophie* (philosophy of life). Up to 1933 it had less standing with the academic world, though no small influence (108). After the NS had seized power, *Lebensphilosophie* gained a controlling position in most universities.

3. *Effects of Klages' Anti-rationalism.*—Nietzsche's disciple, Ludwig Klages, is more radical than the earlier representatives of *Lebensphilosophie*, like Dilthey, who considered consciousness and reason as mere functions and derivatives of the original unity of mind and body (9). For Klages the intellect is a superimposed and hostile power, asphyxiating the originally intuitive and prophetic mind of primeval man and culture. Klages 'proves' over and over again that the only path for psychology is to turn away from rationalist and causal proce-

dures to the primeval level, where symbolic relations between original givens and their expression can be obtained by 'divination.' Klages advocates a revival of the symbolic language of primitive myth and folklore in order to provide an instrument for his *Ausdruckslehre* (science of expression) in the fields of graphology and bodily expression (81, 82, 83, 84, 132).

The amazing popularity of graphology among German academic psychologists is mostly due to Klages' work in this field. Klages' romantic irrationalism and anti-rationalism, like Spengler's, was, moreover, a powerful factor in paving the way among the educated of the generation after the first war for the later NS doctrine.

The names of Klages and the Existentialists are referred to over and over again in German psychological writing after 1933. One might say that besides Race, Folk and Heredity, three concepts derived more or less from political ideology, *Schau*⁵ (approximately contemplation), *phenomenologisch*, and *Ausdruck* (expression), are among the fundamental philosophical terms in the vocabulary of present German psychology.

All of these movements were originally scientific by virtue of their intellectual probity, and even empirical in their scope, at least in the more general sense of the term. Their approach, though in principle anti-physicalistic and intuitionistic, still coexisted with the more exact sciences, and only some of their protagonists can be said to have shown a clear affinity to NS ideas. It was for special reasons that a fruitful interaction between these tender-minded phenomenolo-

gists and the tough-minded physicists did not develop into a democratic give and take, where the theorists would have furnished the hypotheses for verification by the experimentalists. One of these reasons was the supercilious incoherence with which the phenomenologists accepted Dilthey's dichotomy of 'understanding' and 'explanatory' (analytical) methods in science. Failing to realize the scope and the limits of their method they unwittingly helped to lay the field open for outside influences, as will be described later.

4. *Prominence of Characterology.*—After 1933 characterology became the most favored topic in German psychology. In this field the aforementioned universal trends and the specific political ideology of NS converge. The study of the 'total' personality by means of the *Ausdruckskunde*, the interpretation of different modes of expression as indicative of dominant character traits offer again and again an occasion to affirm the unity of mind and body. Moreover, this approach gave aid and comfort to the political doctrine by introducing into psychology a system of pragmatical and, indeed, completely arbitrary values such as the 'personality of the born leader.'

In special sub-fields of characterology the sudden increase in the quantity, though not in the quality, of research is even more patent. Typology was universally applied because the concrete wholeness of a psychological type is expressed in bodily as well as in mental terms, that is to say, typology is 'psychophysically neutral.' Typology sets out to establish the hidden regularity in the concomitance of traits, of modes of expression and of the physical build. The individual case, it is supposed, can be subsumed under the type in such a way that predictions can be made from observable traits to the elementary nature of the individual, once the type has been established.

⁵ Max Weber's grim dismissal of the advocates of *Schauung* as a method should be called to memory: "Wer schauen will, gehe ins Lichtspiel." (Who wants to 'look' should go to the movies. Untranslatable pun as *Schauung*—visionary contemplation—is derived from *schauen*—looking.)

Here again the tendency to evaluate is significant (13, 31, 62, 67). The immediate establishment of 'good' and 'bad' types often becomes the most important thing. This tendency is still more evident in the importance given to *Volkskunde* and *Rassenkunde* (the study of folk and race) which investigate the characteristics of the folk or race reflected by the individual, while *Erbwissenschaft* (heredity) points out the ascendance of certain psychological traits from one generation to the other within a given race. It goes without saying that in these attempts the premises were taken over without modification from the racial-political theories of NS. The existence and supremacy of a Nordic race is a sacred axiom; the primacy of the folk-group over its individual members, the doctrines of the purity of race, the determination of the individual character by its race, all these principles are dogmatically accepted. Psychologists have built these assertions into their systems and 'proved' them through various experiments. A small number of workers in this field have sidestepped the issue, but have not dared to take exception openly to these doctrines (e.g., 155, 156, 157).

5. *Concept of Structure and Organic Wholeness.*—The fundamental principle of characterology—and to an ever increasing degree of psychology in general—is the *Strukturbegriff* (concept of structure) and, closely connected with it, the idea of *organischer Ganzheitlichkeit* (organic wholeness [119]). Both concepts date back before the NS regime, but both achieved universal recognition under it. Whereas 'wholeness' was the methodological principle common to almost all schools and characteristic of the more recent German psychology in general, the idea of structure (meaning not only the blue-print of the interrelation of the individual elements of a complex whole, but also the dynamics of their

interaction) is connected more specifically with the name of Felix Krueger.

Krueger is probably at present the most highly regarded person, the Dean of German psychology. Always a conservative, he was one of the early sympathizers with the NS movement, and says of his department proudly: "The psychological Institute (at Leipzig) was reputed to be a 'folk-cell,' " that is to say, a germinating center for ultra-nationalist activities (102, 103, 99). In spite of his adoption of many of the ideological implications of NS, Krueger is, nevertheless, insistent on the scientific character of psychology. The papers of his school, though showing clearly the effect of the general tendencies present in German psychology, usually stand out in their scientific seriousness, at least as far as their experiments and empirical observations are concerned (101, 105, 152).

Krueger's own theory of feelings is known in this country through its presentation in the Wittenberg Symposium (104, 97, 98). The *Neue Psychologische Studien* edited by him and his disciples are characterized by a usually fruitful combination of genetic and Gestalt principles. A good example is the volume on infant development (18, 61, 96, 100), putting emphasis on the evolution of 'objects' in the child's behavioral world and dwelling especially on its most primitive stage, where complex, 'physiognomic' perceptions determine the child's reactions. One of the best achievements of the School is Otto Klemm's work on motor adaptation, which belongs in the field of applied and physiological psychology.

Integral Typology.—The idea of visualizing personality as a complex of integrated levels is derived from the principles of structure and wholeness, and extends from the 'lower' (physiological, primitive-sensory) to the highest intellectual functions. Such a view is the

basis of E. R. Jaensch's *Integrations-typologie* and of other typological and characterological systems. The *Integrationstypologie* is a far-reaching enterprise, for it has set out to prove on a large scale that every aspect of the entire psychophysical personality exhibits homogeneous features characteristic of the specific type to which the individual belongs (69).

The *Integrationstypologie* was developed from Jaensch's studies on eidetic imagery (64) and thus belongs also to the period preceding Hitler's ascent to power. However, it was in the NS Germany that Jaensch expanded this theory in complete accord with the doctrines of the party to which he gave open allegiance before 1933 (25). Until his death in 1939, he probably wielded more power in psychological matters than any other German. There was hardly a volume among the leading psychological periodicals without a paper from one of the numerous collective studies which he directed (63, 65, 67, 68, 70, 71, 111). Jaensch rejected the 'highbrow' attitude of the philosophy-minded *Geisteswissenschaftliche Schule* (Dilthey) as well as the physicalist approach of Ebbinghaus. Psychological Anthropology was for him the real and fundamental science and the presupposition for all other sciences. In the XIII Convention of German Psychologists, the first to be held under the new regime, Jaensch reported on the *Gegentyp* (counter-type) which thereafter developed into a stock-in-trade of German psychology (62). The *Gegentyp* complements the integrated types previously established, and includes everything that is opposed to genuine German character and to ideas of NS. The *Gegentyp* is thus essentially *lytisch* (disintegrating), showing a tendency toward tuberculosis and schizophrenia, and extending from the pole of extreme softness to the pole of intellectual rigidity and artificiality. It has dominated,

Jaensch held, the cultural life of Germany for the last two hundred years and has produced as its typical specimen the *Erkenntnis-Liberalist* (liberal intellectual), who, adopting a position of empty and bloodless objectivity, *only* contemplates.

The goal of psychology, however, is not only to understand but to *act*. In numerous studies Jaensch's types were matched with other typological systems and examined from the most divergent points of view. Thus it was found that children in certain stages of their development (puberty) regularly go through S-stages—'S' being the counter-type—whereas the true, innate S-type tends to hail from mixed races. The system of intelligence testing as worked out for Germany by W. Stern, was found to be of a structure naturally akin to the S-type, and gave this type undue advantage in competition with the good J-types (66, 13). The J-types, of course, show the characteristics that Günther, Clauss and other NS students of race have assigned to the German folk. Part of the attraction of the *Gegentyp* seems to lie in the facilities which it offers for doing away with any undesirable rival school of psychology. Thus Jaensch demonstrated 'conclusively' that the principles of *Gestaltpsychologie*, the founders of which had emigrated, showed all the marks of the S-type (62, 68).

The pursuit of characterology in Germany always has tempted its students to neglect the critical methodology of science (151). Tendencies operative both in the Nazi ideology and in the general cultural currents of the time prior to the former's ascent to power have favored an unlimited subjectivity of interpretation, and have revived at the same time perennial patterns of speculation about the nature of man. Among the amazing number of books and papers on *Charakterkunde*, *Typenlehre* and *Ausdrucks-kunde* published since 1933 (29, 30,

38, 39, 53, 54, 95, 119), there appears frequently a combination of rabid diletantism with intellectual presumptuousness and political doctrinarism. Under these psychologists fall W. M. Schering as well as most authors of books on race (22, 23, 24, 32, 79, 124, 142). Though not a typology, Herman Poppelreuther's Political Psychology based, according to the author's proud announcement, on *Mein Kampf*, should be mentioned here (131).

The name Empirical Anthropology given by Jaensch to his psychology has significance. Phillip Lersch, originally a follower of the older *Lebensphilosophie* of Dilthey's (108), and one of the younger characterologists whose career falls largely in the period after 1933, distinguishes in Jaensch's teachings (1) the psychological aspect which deals with the actual instantaneous phenomena of the mind, (2) characterology which deals with the more enduring structures, directions and dispositions and, finally, (3) anthropology, which considers the nature of orientation of consciousness and of the existence of the self within the world at large. Higher mental phenomena such as love, despair, awe, as well as memory and imagination, can be understood only in the light of anthropology (106, 107, 110).

For the endorsement of the views set forth in one of his papers (109), Lersch invokes Scheler, Jaspers, Spranger, E. R. Jaensch, Rothacker, Klages and Heidegger, a list which, in fact, comprises an almost complete register of the primarily nonpolitical influences upon contemporary German psychology. Lersch is concerned with the problem lurking behind intuition, the 'divination' of symbols and the rejection of scientific reason: how does the characterologist know that the phenomena which he beholds are genuine and essential and not ephemeral qualities only? In other words,

what are the criteria of the correctness of his interpretation? While the question is well posed, Lersch's answer does not transcend the customary argument of Intuitionism (see also 44).

G. Pfahler, another of the younger characterologists, heads a school which attempts to define elementary functions and structures (such as perseverance and attention) which, because of their apparent independence of environmental influence can not be regarded as acquired but only as inherited. These ideas evidently go back to the Müller-Pilzecker experiments on forms of reaction. From these basic functions Pfahler constructed a *Vererbungstypologie* (typology of inheritance) which, because of its endorsement of the NS ideas on heredity, has been widely acclaimed (93, 94, 95, 125, 126, 127, 128, 129).

7. *Military Psychology*.—*Wehrpsychologie* (military psychology), dates back to the period before 1933. Since that date it has had a phenomenal development. Many institutes, connected for the most part with army centers, are occupied exclusively with topics of military psychology. The number of books and published papers has increased steadily; in the annual conventions of psychologists *Wehrpsychologie* was given first rank. Most psychologists, indeed, were only too glad to turn their minds to problems which might be of interest to the army. Curricula were developed and training courses set up for military psychologists.

In the selection of practicable psychological ideas the commanding group of military psychologists was guided by a shrewd syncretism and restricted by no prejudice. According to information, both the army and the Ministry of Propaganda make use of psychoanalytic points of view in their psychological work, unperturbed by the fact that Freud's writings were publicly burned in

1933.⁶ Generally ideas and methods were adapted to military purposes with utter disregard of the fact that the very ideas were being decried at the same time as outlandish abominations by the rank and file of German psychologists. It seems that as a rule every idea was given a trial but only those were selected which to some degree had proven successful.

The topics of *Wehrpsychologie* extend from the detailed testing of aptitudes in the selection of technical specialists to the study of military leadership (1, 5, 6, 7, 15, 46, 55, 60, 113, 114, 120), and from there to metaphysical explorations of the meaning of mortal fear and the defense against it (112, 115, 130, 141). Psychological diagnosis held the center of attention for some time (90, 91, 92, 117, 121, 136, 149, 164). M. Simoneit, one of the leading figures of present military psychology, enumerates four steps in the methods of investigation practiced: study of the testee's biography, analysis of his intellectual abilities, analysis of the testee in action, and investigation of special abilities (145).

During a meeting of academic psychologists in the ministry of war in 1937, Simoneit made the following classical statement: "Only when psychological diagnosis is performed in the interest of the state, does psychology become really important. . . . Psychological diagnosis for a long time will remain the problem which dominates almost exclusively the entire research in military psychology (169)." The real value of psychological work in the army, however, is still an open question. According to the opinion of one expert,⁷ the influence of army officers who introduced psychological ideas into the army, such as Colonel Voss and Colonel Blau, must be distinguished

from that of the leading army psychologists, men like M. Simoneit and K. Pintschovius, who, according to the information quoted, failed to impress the army because of the unsatisfactory quality of their work.

Characteristic of the methods of military psychology (34) is the combination of the older aptitude measurements with the new characterological approach as it is designed for military purposes. Apparatus developed in N. Ach's school (2, 3, 4), such as J. B. Rieffert's multiple choice reaction instrument, are being used for the testing of officer-candidates; motion pictures of the facial expressions of the subjects are taken through a one-way screen during the experiment. In the final evaluation no more attention is given to the scores of aptitude performance than to an analysis of the candidate's expressions, made on the basis of Lersch's system of physiognomy.⁸

Significant insights into the fundamental dynamics of NS can frequently be found in those papers on military psychology which are concerned with the problem of the nation's readiness for war. Thus, Mierke and Lüderitz, appraising the influence of the experience of the last World War—in the second volume of papers on military psychology—(15) state 'with sober frankness' that the new ideology of *Gemeinschaft* (companionship) in the NS state springs from the "deepest existential roots, . . . the impact of horror . . . the feeling of being helplessly delivered into the power of materiel." This means that the ideology of companionship is based on the crushing experience of utter helplessness and chaotic anxiety in the face of a rampage of metal and fire, an idea which, first expressed by Ernst Jünger (76, 77, 78), was quite influential with the younger officers.

⁶ Personal communication from Dr. Ernst Kris, New School of Social Research.

⁷ Personal communication from Dr. Hans Rosinski, Fletcher School, Tufts College.

⁸ Personal communication from Donald Brinkman, Swiss military psychologist.

However, current interpretations of German military psychology would be misleading, if they overemphasized the role played by a purely qualitative approach. Contrary to conditions in most other fields of German psychology research workers in the services probably use considerably more statistics than they want to reveal in their publications. Concepts like *Funkfrommheit* or even *einfühlende Hingabefähigkeit*^{*} (92, 116, 117) are often used conveniently as protective screens to befog hard-headed quantitative research going on at the same time. The fact that these fields of psychology were 'mobilized' long before the war, can well be the reason for the scarcity of reference to the other type of research such as Masuhr's report of his work on Contingency-Tables (using Hollerith machines) (169; see also 41).

8. *Industrial Psychology*.—*Arbeits-Psychologie* is, of course, contributing more and more to problems directly or indirectly connected with the war. It should be noted that in this field too, formerly sober and matter-of-fact characterological, as well as racial and hereditary considerations, have been adopted on a large scale (10, 11, 20, 31, 45, 60, 118).

Conservative psychological concepts, like Kraepelin's *Arbeitskurve* (work-curve) still hold the field, although re-defined as *Ganzheits-Tests* (holistic tests) of the worker's personality (123). The numerous painstaking studies on quantity and quality of output (e.g., 17, 28, 47, 48), as well as the work on problems of fatigue (e.g., 40), show either direct or indirect influences of N. Ach's 'will-theory' and its exact methodology (2, 3, 4, 154).

^{*} Approximately 'pious devotion to telegraphy' and 'disposition for empathic self-abandon.' The peculiar touch of technical terms blended with sentimental ones, cannot be accurately reproduced.

There is evidence that some of the studies in industrial psychology are subservient to political purposes. For example, there is a tendency to minimize the ill effect of monotonous high-speed work-performance (8, 134).

This tendency is clearly shown in the attitude which appears in discussions about aptitude testing and the selection of personnel. Also it seems to be universally agreed upon that 'selection' means rejection of those individuals who are not fit to serve the 'whole' of the 'community.' However, there should be no concern for the solution of the individual's vocational problem or for the promotion of his personal happiness. A typical paper by H. Röckel (135) argues pointedly against the selection of the exceptionally gifted from lower social levels for professional careers. This selection would mean that they would be taken out of their 'organic social soil,' and this would dangerously deplete the number of N.C.O. candidates of better intellectual capacity.

The large amount of research done on individual motor performance offers a different picture. Most of the publications in this field could be equally well classified as straight psychophysics. Here the idea of *Ganzheit* (wholeness) proves to be of great heuristic value, particularly in the work of Otto Klemm. His research on the Gestalt-character of highly trained motor sequences (86, 88, 35, 122), particularly in athletics, shows that holistic principles can enhance the accuracy of scientific descriptions.

However, as soon as Klemm goes beyond these concrete problems and attempts to define his own (and the Leipzig department's) attitude towards industrialism, the ambiguity of his concepts leads him into paradoxical statements (87). His 'Reflections on Psychological Adaptation' (85), originally planned as an address to the XI. International Congress of Psychology, 1936,

deplores the loss of the 'total,' 'organic' connection of man and nature through the interference of an 'industrial landscape,' a man-made *Zwischenwelt* (intermediary world). Klemm's faint attempts at an 'integration' only underscore one of the fundamental conflicts in NS ideology: the contradiction between an expanding industrial imperialism and the romantic resentment of a world transformed by it, as well as the nostalgia for a world irretrievably gone.

9. *Physiological Psychology*.—In the field of physiological psychology there have been no conspicuous changes during the Nazi era owing to the fact that political influences decrease as psychological research turns to more specific, socially indifferent, problems (14). With the approach of 1939 and the beginning of the war there was a certain narrowing of scope owing to the general preoccupation with military psychology, such as studies on fatigue, experimental analysis of special military performances like range-finding (42, 91), aiming (113, 150, 153), auditory acuity (41), and protection against noise (144). The standard of work done in these fields, mostly under the direction of W. Wirth (159, 160, 161, 162, 163), is traditionally high. The fact that these studies are undertaken entirely in the service of military organizations and hence are tools of the NS policy of expansion add to keep them on an efficient level. Rewards being greater, the number of scientists working in this field has probably increased.

Theoretical work in physiological psychology, especially on perception, continued, under the leadership of V. von Weizsäcker (155, 156, 157), in the line developed in the twenties by the early Gestalt school. The summary of his studies was published in 1940 (155, 12). His theory was developed through the analysis of experimental findings on optokinetic dizziness. The older Gestalt

school, in Weizsäcker's opinion, did not overcome the dualism between stimulus and response in the analysis of perception and movement, and left unbridged the gap between physiology and psychology. In its place Weizsäcker proposes to put a triadic scheme of stimulus, movement and perception—the latter two being exchangeable in certain specific situations. In the studies of his pupils it becomes evident that in this development psychology has become a special field of biology, and that biology in rejecting the causal principle on which the other natural sciences are based, puts itself in opposition to them. With the exception of these latest developments which were influenced by epistemological considerations, Weizsäcker's physiological and clinical work is clearly related to, if not often dependent on, Kurt Goldstein's work in this field, some of which dates back to the twenties.

A more radical formulation is presented in Erwin Straus' *Vom Sinn der Sinne* ('On the Meaning of the Senses,' 148), published in 1935. His position is that of extreme anti-physicalism: *Empfinden* and *Erkennen* are basically different. *Erkennen* (knowing) sets up the universe of natural sciences on the basis of causal structure, whereas the deeper and more genuine level which man shares with the animal, *Empfindung* (sensation), is structured in symbols and expression and does not proceed in the punctiform physical time but in the biological time which, similar to Bergson's *durée*, reaches out from the present toward past and future. Heidegger's *Existentialphilosophie* appears again clearly in this theory which its author thinks to be particularly suited to the description of psychopathological states.

Investigations centering around the old von Hornbostel-problem of intermodal sensory qualities (unity of the senses) have been carried on and usually showed high standards of work-

manship. H. Werner's former collaborator, K. Zietz (168), did work in this field, while several other investigations were contributed by Hungarian psychologists (143), and published in the German *Zeitschrift für Psychologie*.

The monumental work of E. von Skramlik on the 'Psychophysiology of Touch,' published in 1937, represents research which he carried on for several decades, and is by virtue of its topic unaffected by the trends of the day (147). It thus strongly contrasts with the flood of papers set forth by E. R. Jaensch and his pupils, urging a 'revision' of the theory of perception in the doubtful light of their typology.

10. *Psychoanalysis*.—Psychoanalysis had never been well accepted among German psychologists. Jung's romanticism (75) and the parallelism of some of his ideas to Klages' had some appeal for them. Adler's *Individual-Psychologie* found response after it had been modified by the articulate Künckel and transformed into a dialectic system of psychotherapy. However, Freudian psychoanalysis had always been cold-shouldered if not openly spurned. When the Berlin Psychoanalytic Association was disbanded soon after the NS seized the government, the *Deutsche Verein für Psychotherapie* was set up on its premises with the aim of bringing the schools of Jung, Adler and Freud under one roof and eventually moulding the divergent systems into one. Freud's books were barred from open display in book-stalls but until 1938 could be purchased without any difficulty if desired. After the absorption of Austria, Professor Göring came to Vienna in order to convert the Viennese Psychoanalytic Institute into a unified establishment similar to the one in Berlin. The Austrian Nazis, intent to outdo the Germans in radicalism, however, insisted that the pernicious breed of psychoanalysis be eradicated once and forever, and had

their way not only in Vienna but also brought about a reorganization of the Berlin group, in the course of which the remnants of the psychoanalytic movement disappeared completely.¹⁰ In academic psychology the fight against psychoanalysis, and against Freud in particular, was continued after 1933, e.g., by Krueger and Ach (3, 99), the primary argument being the alien, outlandish, and destructive character of the psychoanalytic doctrine as well as its mechanistic and hedonistic premises. Of psychotherapy in general, the new order demanded that it become more conscious of nationalist politics and perform its work in close coöperation with the ruling ideology and its exponents, i.e., the storm-troopers, the Hitler youth, etc. (89).

11. *Conclusions*.—In an appraisal of the general situation of psychology in Germany it must be kept in mind not only that romantic-nationalist ideologies had beset German psychology before 1933, but that many of the developmental forces now prominent date back much further. The political changes, however, have brought into the open what before lay in obscurity. As the most immediate effect the NS tenets of Folk, Race, Community, Heredity, etc., have been adapted for psychological work. Other concepts, like the NS version of organic totality, the 'doing instead of knowing' (142), and its pragmatic metaphysics, served to reinforce similar trends already in existence. But not all the papers and books published in Germany were as foolhardy as Jaensch's illfamed comparison of the northern and southern chick, by means of which the true integration of the Nordic type is favorably contrasted with the Mediterranean type, and an argument derived for a regional theory of race.

The iniquity of the New Order shows

¹⁰ Personal communication from Dr. Edward Bibring, Tufts Medical School.

up much clearer in the book reviews where every publication concerned with anti-intellectualism or any of the other NS articles of faith such as Nordic superiority is reported without a shadow of criticism, and is duly extolled. But in the three technical journals of rank there were, up to 1939, a number of papers which ought to be judged in terms of German rather than Nazi psychology.¹¹ However, the number of papers centering around one or more of the new doctrines, proving whatever their authors wish to prove, and quoting Hitler and Rosenberg as scientific references, has been increasing. The language of psychological publications also seems more pompous and flowery than before. There is a tendency toward grandiloquent concepts, as well as toward neologisms, a peculiar mixture of sentimentalism and toughness, and a profound revelation of the spirit of NS (e.g., 56). The inclination for wider perspectives, a disposition characteristic of German psychology at all times, and

¹¹ In this connection, see Eugene Lerner's interesting study (*J. Psychol.*, 1942, 13, 179–192). He observes a sharp deterioration in the quality of German psychological writing as the expression of mental confusion and severe disturbances of mental health. Without doubting the latter, it must be said that the author's selection tends to be misleading. Of 35 papers mentioned, only four were published in the *Zeitschrift für Psychologie*, the best reputed journal in the field, whereas 16 of the papers mentioned originated from the *Zeitschrift für pädagogische Psychologie* which, from the point of view of scientific psychology, was always considered to be somewhat 'marginal.' The tradition of this periodical, its readers and its editors account for the docility toward the NS doctrine and the new style in science. As a rule, the proceedings of the *Deutsche Gesellschaft für Psychologie* give a much worse impression than do the periodicals just mentioned. The conventions since 1933 are developing more and more into political spectacles. It is probably quite significant that the desire to conform to the new faith and to outdo one another in the display of loyalty to it seems to reach its peak on such occasions.

endowed with virtue as well as danger, is now running rampant.

Closely linked up with the political and methodological dogmatism is an intellectual isolation, which has been growing since 1933. It was probably effected as much by political prohibition barring, for instance, the attendance of scientific meetings outside of Germany, as by the individual rejection of everything that seemed to take exception to the new German faith.¹² German psychology itself has accomplished a violent and arbitrary synthesis of formerly discrete philosophical, psychological, and socio-political trends. But in this process its boundaries are disappearing and psychology is thus becoming a mere aspect of a superordinate, politically-minded *Geisteswissenschaft* or general anthropology.¹³

Whatever is acceptable in the publications since 1938 has its roots in the psychological insights of a much earlier time. The war situation probably has kept German psychology from further disintegration by demanding from it contributions for concrete needs and thus coercing psychology into a more realistic attitude. But the effect of NS on psychology can on the whole be described as an increase of unscientific and irrational attitudes in the various fields of psychology, the aforementioned exception notwithstanding. By 1940 the study of personality had suffered most while straight physiological research, except for the shift in the selection of topics seemed comparatively unaffected.

The changes German psychology has undergone reflect general social trends and their manifestation in the form of

¹² According to S. W. Fernberger (36, 37) the political changes in Germany up to 1935 have caused a decrease in the total number of papers published.

¹³ In this connection the views of a German psychologist on America and American psychology are interesting: Walter Beck, *Grundzüge der amerikanischen Psychologie*, *Z. angew. Psychol.*, 57, 1939, 241–276.

ideologies. The development of German psychology as well as that of NS as a whole may be explained on the basis of the following facts: (1) Recurring traditional ideologies such as Romantic Nationalism; (2) a deep-rooted social pattern, shown in the significance of authority, discipline, subordination, reinforced by kindred institutional patterns; (3) economic problems; unemployment, and the depression causing wide-spread impoverishment and a pervasive feeling of insecurity; (4) the political effect of the period from 1918-1933, or the ineffectualness of the Weimar Republic; (5) the psychological dispositions of the average German at this time, that is to say, typical traits and patterns of reaction such as would be formed under the impact of factors described in the first four points; (6) the dynamics of groups and masses, especially under conditions of radical social change. Obviously, this is only a general and incomplete outline of what appear to be the outstanding trends converging in NS. They will have to be pursued in isolation as well as in their interaction in order to make possible the understanding of an event which, in spite of its many facets, has been taking place as a unified process, and in the end can only be comprehended as such.

Owing to the scope of this paper the discussion has to be limited to the effect of certain ideas which can be traced in the recent development of German psychology. It will be understood that, in doing so, one factor quasi-artificially is being isolated from a complex, and stressed. Even in their abstractness, though, these ideas may still reflect the variety of social causes from which they were conceived.

The ideological current of which *Phänomenologie*, *Lebensphilosophie* and *Existenzialphilosophie* were discussed as outstanding examples, certainly con-

tains in many of its publications conspicuous elements of the later NS ideology. Since these ideas were of a wide appeal, it is an open question how much they helped toward preparing the intelligentsia at least for the acceptance of the Nazi-System. For psychology, in any case, they had, prior to the rise of NS, laid out an ideological framework which had only to be consistently extended in order to produce the scene described in the preceding sections. Irrationalism, biological mysticism, subjectivist and romantic values implying the rejection of humanistic and equalitarian ideals, the discard of the traditional standards of scientific evidence and a peculiar kind of phantasmagoria, were the fundamental features of these philosophies. At the same time it must not be overlooked that in this ideological current there were also ideas which in themselves were politically absolutely neutral.

The influence of these schools which, for short, will be called 'intuitive,' was growing even before NS came to power. Their effect, however, was balanced by many divergent ideas so that at this time their future role could scarcely have been predicted. *Phänomenologie*, *Lebensphilosophie* and all the writings in the field which showed their influence, were, above all, *philosophies* and *psychologies*. Whatever ideas of later political prominence they might contain, they still were overwhelmingly technical, and thus politically indifferent. *Phenomenologie* and *Intuition* as *methods*, that is as principal procedures of investigation are, of course, in themselves non-political although this does not—and did not—preclude their employment for political purposes. It seems indeed likely that their existence and spiritual influence under the specific circumstances of this time effected the political development in Germany in various indirect ways. The political usage of an ideology which, in

itself politically indifferent, is again predicated by social (and thus political) trends, is in a high degree characteristic of the complex interaction between ideas and their cultural and social matrix.

Not all the ideas in line with NS in German Psychology before 1933 are found in the writing of the Intuitionist School, nor do all intuitionists adhere to these ideas. Both types of ideas, however,—the ones coming into psychology from outside as the political creed of certain psychologists (e.g., Jaensch) and those which were immanent to certain systems (e.g., Klages),—should be considered in their relation to a much more universal trend in Germany history.

A broad continuum of political ideas and attitudes can be traced from the German Romanticism of the early 19th century (165) to Paul de Lagarde and Houston Stewart Chamberlain, and from there to Oswald Spengler, Moeller van den Bruck and the theoreticians of NS (166). It is essentially a brand of romantic nationalism and social conservatism, whose influence has varied with the times but which has always been operative. Kindred ideas found in the writings of *Phänomenologie* and *Lebensphilosophie* should be considered as manifestations of this current.

With NS a political party came into power which took its premises from this romantic nationalistic trend. Those ideas in Psychology which fitted this pattern, received a tremendous backing from the rapidly expanding dictatorship. Naturally enough, nobody bothered whether the relationship was genuine, or apparent only. Neither was it an accident that many of the proponents of ideas in psychology belonging to the sphere just characterized had been sympathetic to the coming NS party all along the way, so that no closure had to be established between scientific and political ideology because in many instances it had already been well worked

out in advance. In any case, to hold the right views in psychology now became a vital matter for many psychologists. To pay homage to the official doctrine was soon the only way to win the favor of those in power.

Opportunism was certainly a major motive of the hurried conversions around 1933. However, it does not seem that the vagaries of the subsequent development can be explained on this ground only. The fact remains indisputable that an increasing number of German psychologists evidently believed in the new NS props of their psychologies as well as in the methods through which they made them come true. One explanation for this fact should be sought in the basic irrationalism of the NS movement, which permeated all aspects of German life (33).

Some of the non-political, technical ideas attained universal favor—e.g., the idea of *Wholeness* (19) or that of *Strata of Personality* (137)—both valuable concepts of no natural relationship to NS. The explanation seems to be that they belonged to the complex of ideas which had been characterized as 'intuitional' and thus were advanced with the rest. This explanation also seems to hold in part for the method which had helped to establish these views, which are primarily *Phänomenologie* and *Intuition* (56). Under the impact of NS they were quickly whipped into such a shape that they would produce anything the ruling doctrine demanded. Herein seems to lie one of the most important reasons for the prominence of these methods in German psychology under NS.

Prior to 1933 *Phänomenologie* and related schools had already shown a tendency to lose sight of empirical reality. While 'direct evidence' was sought through these methods, empirical verification was dismissed as obsolete. Thus scientific truth became a function of the

individual observer's truthfulness, and identical with the genuineness of his experiences (feelings). Truth had been anchored entirely in the individual, and was supposed to unfold in a region which hardly lent itself to critical examination. When, in spite of the endeavors at *Schauung* (intuitive contemplation), no *absolute* truth 'unravelling itself,' the difficulty inherent in such a position increased. It had been *the* problem of the period since 1918 to set up reliable standards and firm beliefs in a world where nothing seemed to be certain any more. Strength of feeling meant evidence, and evidence meant security in a mounting chaos. Thus a bewildered and exasperated mind was inevitably swayed to the doctrine which by sheer virtue of its primitiveness *seemed* strong, and without doubt, in its influence on many *was* strong. The method obediently followed suit. There was no opportunity for that critical examination of the fundamentals of NS, which one might have otherwise supposed. The attitude of ultimate reliance on the power of reason had been forfeited and could not be recovered now when it was direly needed. The habit of rational scrutiny had been in discard for too long, and NS, in view of its aims, was more than eager to endorse the position that intellect was 'the adversary of the soul.'

Another reason for the disintegration of methodological standards and for the unabashed assertion of opinions without even a whiff of evidence, as increasingly displayed in the more recent documents of German psychology, was alluded to before. In a pattern of living increasingly emotionalized and regressive the restraint and realism of scientific procedures becomes a danger as well as an offense for the individual who wants to sustain his indulgences at any cost and therefore can not permit his rationalizations to be questioned. Thus methods will be favored which are largely specu-

lative and can be used in such a way that the actual qualities of the reality which they allegedly investigate need not affect them too much. This applies, as mentioned before, especially to those fields of psychology concerned with the common problems of the individual personality and of the group. Behind the universal adoption of the tenets of NS there seems to be a strong tendency to demonstrate with the material of psychology that the NS doctrine *is right*.

But all this, it must be kept in mind, is only *one aspect* in the transformation of a national group on the most tremendous scale. The changes in German psychology cannot be accounted for by the effect of ideas and methods alone. The inquiry into the development of psychological ideas has to be shifted to an analysis of people who, under the impact of affective intoxication and fear are kept in a state of mass behavior, and of whom some happen to be psychologists.

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CONVERGING APPROACHES TO PERSONALITY: MURRAY, ALLPORT, LEWIN¹

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As psychology begins gradually to extend its scope from sensation, perception and other segmental issues to the problems of motivated human nature, certain theoretical emphases are becoming clearly articulate. Even while these approaches all agree as to the ultimate goal of the science, they offer different means of investigation, center attention on different sets of facts and employ different conceptual tools. The present discussion is an attempt to provide a perspective in which certain of these contemporary standpoints may be viewed as essentially convergent and as affording the nucleus of a new discipline, 'personology.'²

This survey includes the work of three personologists who have made signal contributions within the past few years. Others might have been considered in a more comprehensive discussion but in their originality and significance few compare with Murray, Allport and Lewin.

The work of Murray and his collaborators, as presented in *Explorations in Personality* (7), strongly displays a psychoanalytic orientation. It was, in fact, the extrinsic aim of the group who produced this volume to subject certain of the analytic concepts, particularly those of Freud, to critical and experimental scrutiny. More intrinsically their goal was to create a system of personology which would be adequate for the construction of an intelligible psychological portrait of the individual man. The

volume thus includes a scheme of personality variables, theoretical discussions such as that on the importance of childhood experiences, and certain concrete experimental and test procedures which were employed in the study of the young men who served as subjects. The personality is viewed throughout as expressed in, if not equivalent with, its biography. Because of the distinctly psychoanalytic coloring of Murray's approach and in keeping with his own avowal, this volume will be taken as an example of a sophisticated experimental application and extension of the Freudian standpoint.

Allport's *Personality—a Psychological Interpretation* (1) embodies not only a different theoretical emphasis but a different expository aim. Conceived more in the form of a treatise than as a monograph, this book reviews the whole field of personology systematically and presents as its central thesis the personalistic note which its author attributes in part to William Stern.³ Here, too, the emphasis is upon the individual personality—treated, however, more *de facto* than *in statu nascendi*. The argument is militantly set forth that somewhere in the interstices of general psychology's nomothetic concepts the unique individual has up to now been lost; to

¹ Presented in substance at the Psychology Department Seminar, Worcester State Hospital, 1940.

² This neologism is Murray's. It seems well suited to its usage but is open to etymological question because of its hybrid descent.

³ Allport is not appropriately called 'personalistic' in Stern's sense. While definitely influenced by Stern, he has developed his own point of view with even greater stress upon the uniqueness of the individual personality. If Allport's position is characterized as 'personalistic' in the present discussion, the justification is that, barring historical precedence, the term belongs intrinsically more to him than to Stern.

rescue him is the essential business of personology.

The third part of the present trilogy is represented by Lewin with several recent works, notable among them being *Principles of Topological Psychology* (5). Though characterized by certain unmistakably original features, he writes as a leading member of the Gestalt School. In fact, topology—a branch of mathematics—as he has applied it to the problems of personality definitely expresses the basic thesis of this school that total configuration, complete context, must always be considered. By conceiving of the person as imbedded organically within a behavioral field of which he is a dependent part, topological psychology sets about to provide concepts like life-space, barrier, and fluidity of the medium which make it possible to describe changes within the relationships of the field. In other places, for example, in *A Dynamic Theory of Personality* (4), Lewin has presented certain theoretical discussions and experimental researches which lend explicitness to his topological

system and add that important part of his total theory known as *vector psychology*. A fuller name for his point of view would probably be topological-vector psychology but for convenience the shorter expression *topological psychology* will be here employed.

Now, it may be stated in anticipation of further exposition, that these approaches seem to divide the total field of personology among them along certain natural lines of articulation which make their reunion in a broader framework relatively easy. Despite inevitable points of difference and of legitimate controversy in certain overlapping regions, they converge to form a first stable terrain for any future system of personology. The accompanying chart depicts this complementary pattern in abbreviated form.

If the three approaches just briefly introduced are compared with regard to their objectives or, to express it otherwise, according to their protagonists' own avowed conceptions of what constitutes personology, this complementary

THE COMPLEMENTARY PATTERN OF CONTEMPORARY THEORIES OF PERSONALITY

Protagonist— Derivation	Murray— Psychoanalytic	Allport— Personalistic	Lewin— Gestalt
Objectives	Reconstruction of past life experience to explain present organization: thematic biography.	Present organization of the individual personality—as distinguished from other personalities; unique psychograph.	Establishment of general laws of organization within the current behavioral 'field': $B = f(P \times E)$.
Methods of Study	Free association and dream analysis: apperceptive tests of fantasy. The case history method.	Personality tests, judgments and ratings. Intercorrelations of expressive movements by statistical procedures.	Controlled environmental manipulation or experiment.
Leading Concepts	Need, press, thema; libido-theory, complex. Ego-defense mechanisms and symptoms; the unconscious. Personal biography as Gestalt.	Trait: congruence and consistency. Functional autonomy. Uniqueness of the individual: individual personality structure as Gestalt.	Vector, valence, quasi-need; life space, boundary, barrier (topology). Contemporaneous motivation. Uniqueness of individual event: field as Gestalt.

relationship at once appears. For Murray and other psychoanalytically oriented writers, the chief business and aim of personology is the reconstruction of the individual's past life experience in order to explain present behavior. As already noted, Murray makes the personality coextensive with its biography and regards each moment of experience in the history of the individual as a part of his now functioning personality. Thus he writes (7, pp. 282-83): "To conceive of personality as an historic flow or emergence of events is to be directed to the study of past occurrences. Abstract biography is the personality, as far as it can be formulated." And again (p. 722): "... perhaps the most potent reason for selecting the Freudian approach was that we wanted, first of all, to investigate personality as a series of genetical experiences, and Freud's theories seemed to provide, if not the open sesame, the only comprehensive and coherent scheme for dealing with the events and fantasies of infancy." He happily expresses the same thought by comparing the personality with a musical score. Composition of this sort has a definite temporal configuration, discernible sections, specific notes or chords, repetitive themes, and inter-related levels or clefs. Obviously this analogy stresses the dynamic character of the psychoanalytic approach at the same time that it throws the biographical and genetic aspects into relief. The organization which this point of view brings out is a temporal one; the art of life, including its discords, is expressed in the dynamic movements of the individual's moment to moment existence. In such a portrayal the past naturally receives a full share of emphasis since the psychoanalyst's ear like that of the listener at a concert catches present and awaits future events in their relationships with all that has gone before. More than in either of the other points

of view this approach to personology lays the past under heavy contribution to an understanding of the present and the organization—or disorganization—which emerges bears the hallmarks of time. Like the functional school in general of which it is a part, the psychoanalytic system thus stresses successive biological adaptation as a key to the nature of personality.

In considerable contrast is the personalistic orientation embraced by Allport. Here, too, organization is, of course, stressed but that organization is thought of as being largely *un fait accompli*. The roots of the present in the past are of little importance as compared to the contemporary personality organization. Moreover, the uniqueness of this organization is stressed. Allport thinks of the individual person as presenting a highly differentiated matrix that momentarily reaches out to find and is influenced by the world. What comes first according to this formulation is the idiopathic character of the individual. In such a description segmental biological adaptations, past or present, are definitely secondary, though not excluded by any means, to the business of understanding the organization of the personality within its own bounded area. Problems of normal adjustment are emphasized in contrast to the psychoanalyst's chief concern with the abnormal.

The topological point of view in personology, of which Lewin is the protagonist, has still another objective. Growing out of the Gestalt School of psychology, this approach represents perhaps the most radical attempt of all to cope with the problems of personality. The fluidity of the intercourse between individual and environment is presented in bold relief by Lewin's avowed aim to establish general laws of organization within the current behavioral 'field.' The distinction between person and environment is thought of as being an ab-

straction from reality, and topology is applied for the purpose of giving mathematical representation to behavior which is a function of personality *times* environment. In this equation behavior is conceived as occurring regularly within a mathematically defined 'field', of which the personality and the environment are both interdependent members. Here more than in either of the other two personologies under consideration the present environment of the individual is thus strongly emphasized. The equation $B = f(P \times E)$ makes the personality and the environment not only interdependent but more or less equally important in the explanation of behavior. One must therefore expect that environmental forces will play a highly determinative role in the formulations of this system and that these forces will belong to the individual's present experience rather than to something which no longer exists as such.

A glance at the chart reveals that the objectives of these three approaches to personology represent a natural division of labor. Murray and the psychoanalysts are intent upon what the personality is at present by virtue of its past experiences. Here the individual is seen as rooted in his past life though pressing onward to express still further certain basic tendencies and trends which have already manifested themselves before. The past in so far as it illuminates the present and is repeated in the present looms large, particularly where abnormal adjustment is in question. Allport, on the other hand, from the personalistic standpoint remains within the boundaries of the individual personality as it now exists. He seeks for an organization which is not primarily temporal or historical but which does justice to the uniqueness of the given individual as exemplified in his every normal reaction. Finally, Lewin concentrates attention upon relationships of a consistent and

law-abiding character which emerge from the organization of personality-and-environment conceived as existing organically together in one behavioral field. The present environment of the individual—if, in fact, such an expression does not itself do violence to the Lewinian point of view—plays a prominent role in this type of formulation. The personality as such receives a relatively secondary place in the scheme or, at any rate, plays no greater part than do other aspects of the total field.

The avowed specific purpose of the topological system, which is to establish *general laws of behavior*, thus contrasts sharply with the specific aim of the personalistic approach which would achieve a *psychograph of the individual person*; and, again, with the specific objective of the psychoanalytic method which would produce a *biography of the individual* showing dynamic trends, harmonies and disharmonies. Since at any specified time concrete behavior emerges from the interaction between a *personality of unique psychograph*, of which much is attributable to *past experience*, and an *environment*, of which this personality is an organic part, the reconciliation of the topological, personalistic and psychoanalytic objectives is operationally easy.

The *methods of study* characteristically employed by the three approaches may next be examined. The psychoanalytic investigator, as is well known, resorts invariably to free association on the assumption that an uninhibited flow of mental content will bring out certain natural relationships between the present concerns of the analysand and aspects of his previous experience. Even repressed, and hence more or less unconscious, items attributable to earlier conditioning are in this way presumably brought to light. To aid in the elicitation of unconscious material, free associations are often started from the ele-

ments provided by a dream or fantasy. Since these bizarre products of the mind are considered to be causally related to unresolved tensions themselves derived from past experiences, it follows that these aids to free association are like free association itself intended to disclose the effects of the past upon present mental content and behavior. In keeping with this general methodology a chief, if not the outstanding, contribution of Murray's volume is its impressive use of the Thematic Apperception Test (6). In this procedure the subject produces fantasies in response to pictures which he is asked to regard as if they were illustrations in a story. By such methods a dynamic case history eventually becomes possible.

The personalistic investigator, *vis.* Allport, habitually employs none of these instruments. He uses tests, to be sure, but they are rarely tests of fantasy. Attitudes, values, judgments of various types of stimuli are the objects of his tests. In addition, ratings and numerous other devices for assessing the subject's traits are utilized. Statistical procedures frequently play an important role in bringing order out of the accumulated data that the various measuring instruments produce and this is one indication of the fact that the methods of the personalistic approach are much more apt to be quantitative than are those of the psychoanalyst. As examples of what is produced under the personalistic banner may be cited the paper-and-pencil test, devised by Allport and Vernon, intended to disclose a subject's self-judgments regarding the Spranger system of values—economic, political, religious, etc.; or, again, the studies of these same authors (3) in which various expressive movements, including handwriting and weight-lifting, in a constant group of subjects were intercorrelated to determine whether a given individual displays any consistency from one type of expres-

sion to another. It will be readily appreciated that these investigations and the methods employed in them are concerned with the personality as such. Scrutiny is concentrated upon personality factors that are primarily autogenous and these are either measured singly or related to each other in order to disclose the nature of the organization existing among them. The derivation of such factors from past experience is held to be of negligible importance and hence receives scant investigative attention. In like manner the relationships of the personality to the environment in which it functions receive relatively little study and are not much conceptualized. The environment of course figures in order to elicit responses that reveal the nature of the personality but the personality alone is of special interest. The environmental stimuli serve merely as occasions for the exemplification of traits, skills, attitudes and motives.

The topological psychologist, notably Lewin, brings to bear still another set of procedures in his studies of personality. Interested as he is in relationships within the field of behavior his methods consist largely in the application of systematically varied environmental situations to a hypothetically unchanged group of subjects in order to discover what the differences in the situations mean in terms of behavioral regularities. From one point of view the work of Lewin and his group thus approximates much more closely than does that of the psychoanalytic or the personalistic psychologist to what is ordinarily regarded as an experiment in other fields of science. On the assumption that the subjects employed do not change from one occasion to the next and are sufficiently numerous to cancel out important individual differences, it is possible to learn a good deal about the equation $B = f(P \times E)$ by the manipulation of environmental variables. An example or

two may clarify this brief discussion of Lewin's methods. In an experiment by Ovsiankina (8) it was found that subjects, if allowed to resume freely either tasks in which they have previously been interrupted or tasks which they have previously completed, show spontaneous and rather marked behavioral preferences for the unfinished ones. Similarly, Zeigarnik's subjects recalled many more unfinished than finished tasks previously performed in the experiment (9). In both these instances, as in nearly all the Lewinian experiments, the subjects were assumed to remain constant from situation to situation and were regarded as being relatively similar in respect to the development of tension systems. As is readily apparent, however, and as will be brought out more fully later, the comparative neglect of the personality of the subject represents a weakness of this approach. In this respect Lewin's methods present almost a complete contrast to those of Allport since, as has already been seen, Allport stresses the personality as such at the expense of environmental forces. This difference is the more interesting in the light of the fact that in other respects these two personologists have much in common.

The conclusion seems warranted that the methods of study characteristically employed by the psychoanalytic, personalistic and topological personologist, respectively, are appropriately adapted to their special objectives as previously described. To the first or psychoanalytic standpoint the investigation of the past life experiences of the individual, adaptive or maladaptive in nature, is of chief concern and here the tools of free association and fantasy analysis, as subservient to the case-history method in general, plays a chief role. To the second or personalistic investigator, where the organization of the normal personality is the main concern, a variety of quantitative tests, judgments, ratings

and their statistical intercorrelation form the main armamentarium. Finally, the topological personologist, interested as he is in the establishment of laws of relationship between personality and environment within one organically organized field, performs controlled experiments by altering that part of the field which it is easiest to alter. He can then compare one such set of variations with another. No essential incompatibility therefore exists among these three ways of studying the behavior of individuals. It is possible on one occasion to investigate the relatively permanent characteristics of a subject and their intra-individual relationships; on another occasion, to seek the derivation of these characteristics in past experience; on still another, to put the subject having these characteristics in systematically varied environmental situations so as to determine the effect of such changes upon the nature of his behavior.

A final comparison of the three personologies concerns the *leading concepts* which have been evolved by them to help in the *interpretation* of their findings. The psychoanalytic standpoint, including the modifications introduced by Murray, may be first considered. Here the concepts of need or instinct, press—describing certain types of situation that elicit a need—and thema or complex play an important part. Closely related is the theory of libidinal drives and their stages of development. All of these concepts center around the biological ends that guide the individual's behavior and that figure in adjustment and maladjustment. *Abnormal adjustment* is here of special concern and is frequently traceable to fixation at earlier developmental stages and to repetition-compulsion. Frequently such repetition is combined with certain mechanisms of ego-defense and, correlatively, with certain symptoms that are the expression of the individual's failure to make an adequate ad-

justment in the past. The symptoms are conceived as a compromise in which the ego or personality organization is protected while at the same time the unsatisfied drive or needs of the individual, possibly crystallized by now into a complex, gain a modicum of satisfaction. Since such compromises and the complexes and fixations of which they are a result are effected without much awareness on the part of the individual himself, the construct of the unconscious is introduced to make plausible how these adjustments are made contemporaneously on the basis of experiences which the individual once had and now no longer remembers. The unconscious is thus preeminently a genetic construct and like need, thema, complex, fixation and repetition-compulsion emphasizes again the role of the past in the individual's present personality.

Allport's personalistic approach also employs motivational concepts but is unfriendly to variables like need which are considered too general for the uniqueness of the individual. Hence in this view the concept of trait is pivotal. Traits, while regarded as common in certain instances to whole groups of individuals, are conceived as sometimes also specific to the particular individual. Of far greater significance to Allport than the commonality of traits or their similarity from individual to individual is their congruence or consistency within any one personality. Constellations of this kind must obviously be important if one's aim is to describe the special type of Gestalt which that subject exemplifies. The laws of personality which are of importance to such an investigator have much more to do with the evolution of special forms of uniqueness and of organization than with invariable regularities as, for instance, between a certain degree of generalized tension induced by a particular type of stimulus and a certain type of recall. Moreover, the past

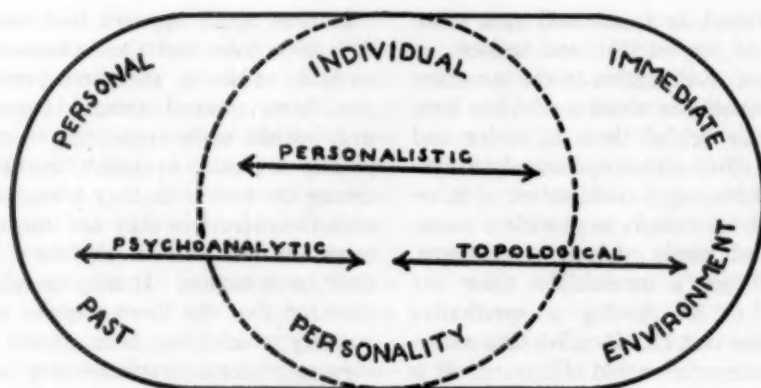
life experience of the individual is conceived to shed little light on the problems of the personality since by virtue of what is termed *functional autonomy* any influences which may have modified the personality at an earlier time become so reorganized and altered in relation to the rest of the developing personality that they lose their original identity. Woodworth took this matter into account in his principle of the transformation of habits into drives. Here again is reflected a deep concern with the uniqueness of individual organization as against those unchanging and procrustean aspects of the personality which, in the purview of the analyst, have failed to keep pace with the rest of development. The concept of functional autonomy is thus almost directly opposed to that of repetition-compulsion, even as the study of normal adjustment is at points naturally opposed to that of the abnormal.

The Lewinian topological system employs concepts which are intended in the first instance to do justice not so much to the unique organization of the personality as to that of the individual event or field of behavior. As already pointed out, the personality is here thought of as having an embedded relationship to the environment within which and with which it interacts. It is the system which this total field exemplifies at any given moment that the concepts of topology and vector psychology are calculated to describe. In other words, the Gestalt of chief interest is not, as with Allport, the uniqueness of the given personality but the uniqueness of the given event or field of behavior. Accordingly, the conceptual tools employed in this approach are vector—which represents the individual's behavior toward an attractive environmental object—and, reciprocally, valence—which describes the action of the attractive object upon the subject. The con-

cepts of need, or quasi-need as a more suitable *ad hoc* variant, and tension, as a resultant modification in the structure of a personality in which a need has been aroused, lie behind those of vector and valence. Unlike the psychoanalytic system, and Murray's modification of it, no attempt is here made to provide a classification of needs, drives or instincts. As in Allport's formulation these are thought of as having a qualitative uniqueness that classification into many-named categories would obliterate. It is regarded as more exact to characterize needs in specific relationships. The topological concepts such as life-space, barrier and boundary, which are accordingly employed serve to describe in non-metrical terms the properties of the field within which the needs, vectors and valences operate. Naturally in such a system any reference to the past would be eschewed. As Lewin vigorously asserts, only contemporaneous motivation can have a place in topological-vector psychology. The past is dead except in so far as it has left traces which now operate as a part of the field. Like Allport, therefore, Lewin reacts against the psychoanalytic stress upon genetic reductions and repetitions. His stand is, however, based not upon the uniqueness of the individual personality—the intention of functional autonomy—but upon a purely psychological and uni-temporal mathematical representation of forces and properties in the present field of behavior. It is thus clear that, while the Lewinian system resembles the psychoanalytic in its invocation of needs and similar dynamic concepts, it resembles the Allportian approach in its emphasis upon unique organization and in the repudiation of temporally reductive explanations. It differs from the personalistic standpoint, however, in its comparative neglect of the personality as such and in its compensatory emphasis upon environmental forces.

It thus again appears that, as with their objectives and their characteristic methods of study, the three personalities have evolved certain conceptual tools which, while conflicting at certain points, are not so much inconsistent among themselves as they are consistent with the objectives they are intended to serve and the methods that have led to their construction. It may therefore be repeated that the three theories of personality which have been passed in review represent a complementary pattern. The objectives, investigatory methods, and leading concepts of the psychoanalytic approach (as exemplified by Murray) are appropriate to a conception of the personality as a *biographical Gestalt*. In it the past naturally plays a highly significant role. The aims, concepts and methods of the personalistic approach (represented by Allport) are appropriate to the personality conceived as a unique *Gestalt within its own boundaries*. Here both past and present environments play secondary roles. The goals, techniques and terms of the topological approach (represented by Lewin) are appropriate to the personality conceived as embedded within a *field Gestalt* of which it is one organic member and where therefore both past experience and the personality as such take a secondary place as compared with the importance of the total field. In it the present environment is underscored. In brief,—and as the accompanying figure shows—while Allport emphasizes the personality as it now appears within its own boundaries, Murray queries how it got that way from past experience, and Lewin studies its interaction with the present environment. As the overlapping arrows of the figure imply, legitimate grounds for fruitful controversy exist, but the general orientations of the three theories admit of easy reconciliation.

Perhaps no better proof of this conclusion could be given than the psycho-



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logical history of the most recent past which finds the three protagonists just discussed coöperating more and more closely. Murray and his co-workers have adopted many of the Lewinian procedures; Lewin has become increasingly interested in the relationships of his system to psychoanalysis and has also begun to elaborate those aspects of his theories which relate to the properties of the personality as such; and Allport has undertaken an extensive research on methods of studying the life-history (cf. 2). In these excursions the best possible opportunity will be provided for testing the areas of disagreement unmistakably present in the three psychologies despite their general convergence. Should the present favorable outlook continue, it will then not be long before the implicit complementary pattern which has here been hopefully depicted will become an accomplished fact of psychological science.

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LOGIC IN PSYCHOSOMATIC MEDICINE

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For many years in the fields of medicine, psychiatry, and in abnormal psychology, there has existed a controversy over the so-called functional or psychogenic, as opposed to the structural, origin of certain disorders such as hysterical anaesthesias. Quite recently the number of these disorders has been increased to include peptic ulcers, mucous colitis, spastic bowel, hypertension, bronchial asthma, migraine, diabetes mellitus, eczema, and arthritis. There is an increasing tendency to regard these and other so-called psychosomatic disorders as end-results of psychological, as distinct from physiological or organic, causes. From the critical standpoint of theoretical psychology any widespread adherence to such a belief may be dangerously misleading.

The idea of functional or psychogenic origins of disease has been the etiological heritage of a long and widely disputed philosophical question upon which opinions have been acutely divided down through the ages. Functional origins have been championed by eminent modern psychologists such as McDougall who once said: "It may be defined as the problem of structure *versus* function. It has sometimes been stated as the question: Does structure determine function? or does function determine structure? It is the fundamental biological problem of mechanism *vs.* vitalism" (4, p. 30).

The problem reaches a new peak with the sponsorship by the Committee on Problems of Neurotic Behavior, Division of Anthropology and Psychology, National Research Council, of a technical journal known as *Psychosomatic Medicine*, to which contributions are made by

reputable psychoanalysts, medical workers who employ psychoanalytic methods, physiologists, and even experimental psychologists. In the first issue of this journal Dr. Franz Alexander (1) of psychoanalytical fame, in a semi-historical article on the psychological aspects of medicine, attempted to resensitize mechanistic thinkers in the fields of biology and medicine to this problem, as McDougall did some twenty odd years ago. Alexander stressed the avidity with which most scientific exponents of medicine wish to retain the mechanistic tradition built up so strongly in the nineteenth century, a tradition which is credited with the rise of scientific medicine to a status of methodological equality with the 'more exact' scientific disciplines. He pointed out the present extreme unwillingness to revert to a position of mysticism and vitalism so prominent in the early beginnings of medicine, which many mechanistic adherents confusedly feel must be done if one is to postulate psychopathogenesis.

The controversy can readily be dissolved by the use of simple operational logic. The argument concerns mainly the difference between structure and function which, otherwise expressed, concerns the difference between body and mind. It should be unnecessary here to reiterate the fact that no differences have ever been distinguished which may serve as universal criteria of either body or mind. Consequently, as Pratt states, "*If it is impossible to tell the difference between the mental and the physical, then the problem of the relation between them need never arise*" (5, p. 20). This statement implies that some form of monism only can be given

scientific credulity. Further inference reveals that to speak of psychogenic determination of organic disorder as *opposed to* organic determination is sheer nonsense. Both are examples of the same type of determination. There is only one type of determination, or determinism, which is recognized by science and that is of the simple 'if this, then that' variety. Any other apparent difference based on body-mind distinctions is purely at a verbal level. To inquire into the ultimate nature of observable events or relations, from the standpoint of the theoretical scientist, is to be a metaphysical has-been. Certainly there is no place in theoretical science for such an inquisition.

Alexander pointed out that no epistemological dichotomy is necessary if "psychic phenomena [are regarded] as nothing but the subjective aspect of certain bodily (brain) processes" (1, p. 14). Subjective or otherwise, there is no need from the point of view of theoretical psychology to defend any philosophical position with respect to the body-mind problem. At that, Alexander has not necessarily resolved the dichotomy, but has merely circled it with a double-aspect monism. When he speaks of 'subjective aspect' he is, no doubt, referring to such observable events as anxieties, wishes, repressions, etc., which are said to be the cause of spastic bowel, etc. In the past attempts have been made to differentiate these events from certain other events, called physiological, which also have been observed to take place within the confines of a human frame. Knowledge of both has been achieved by observation, so that the facts in both instances are sensory facts and give no clues regarding a 'real nature' which may or may not exist (solipsism not intended). Nor do such facts render plausible a conclusion that the 'nature' of a receptive wish is either the same as, or different from, the nature of

a change in the hydrogen-ion concentration of the blood.

When viewed operationally, causes turn out to be no more than observed sequences of events and whether any such sequence is of a different 'nature' from any other need not enter the picture to confuse the issue. To say that anxieties cause peptic ulcers is to say no more than that a sequence of events was observed in which anxieties preceded the appearance of the ulcers. Further evidence for a causal relationship has been found when, by removing the anxieties, it was discovered that the ulcers disappeared. This type of observation is analogous to the removal of portions of a rat's brain and the observation of a subsequent change in learning behavior. There should be no need for concern over how a psychological entity can produce a change in a presumed ontologically different physiological item or event, or vice versa.

It is felt that confusion arises over the seemingly spectacular development of a peptic ulcer, because of the relative inaccessibility or private nature of the so-called psychological antecedents of this physiological change. It is comparatively more simple to detect and 'measure' a peptic ulcer than it is at present to quantify an anxiety. For this reason an anxiety and an ulcer may appear to the unwary to be different from one another in 'nature,' making their relationship all the more startling. When Alexander speaks of 'subjective aspect' he is certainly referring to these relatively unmeasurable or unquantifiable observations. The recent development of psychology as a scientific discipline will, in part at least, explain the present impossibility of dimensionalizing certain of the observable phenomena which fall within the domain of psychology. These phenomena, because they cannot be observed with the same or similarly refined techniques as so-called physiological

events, have, unconsciously perhaps, been grouped into a separate class and have come to be regarded as something fundamentally different. The only difference demonstrable between the two classes is that more has been recorded and accurately measured in one group than in the other, that one group is more accessible than the other with available techniques. This is a difference to be sure, but of a quantitative rather than a qualitative nature.

It may be argued that anxieties will eventually be explained in terms of physico-chemical activities and that the associated peptic ulcers will then be more accurately described in those terms, but physico-chemical activities will never be shown to be the same as anxieties since they are separately observed, and by different methods, as antecedent or simultaneous events. Each event, whether it is termed physico-chemical or psychological, requires a separate sensory observation, so sequences of separately observed events will be all that are available from which to establish relationships and principles.

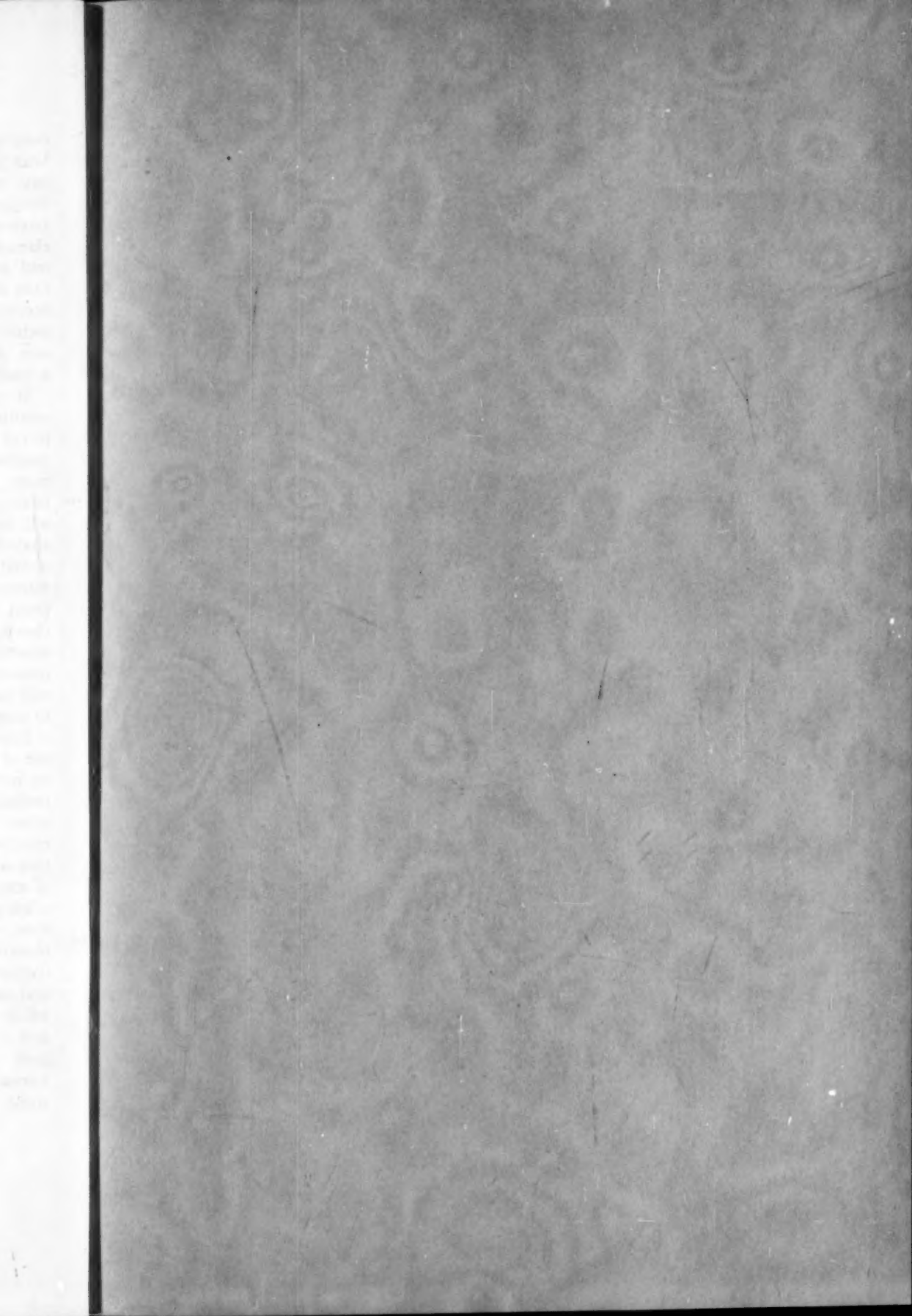
Explanation is arrived at only by the use of logical constructs which are built up out of the facts observed in an experimental setting. These constructs, often mathematically complex, are the relationships, principles, and systematic theories which constitute the only type of explanation properly to be found in science. Too often, and sometimes too soon, these principles constructed from observed facts transcend to an inordinate degree the observations originally made and one is left in a realm of mysticism which may be very misleading. The fact that the body-mind problem has been reared into the field of psychosomatic medicine shows that such a trend is on the way. A perusal of the

articles in this comparatively new but thriving journal will verify this notion. It is to be remembered that transcendentalism has no place in science.

In addition let it be said that causal connection between anxiety and peptic ulcer is not necessarily demonstrated by the case history method which is so frequently used in this field. "Case histories give valuable hints as to what lines of investigation should be followed, but in themselves they are worthless as far as establishing functional dependence is concerned" (5, p. 164). Conclusions drawn, *i.e.*, facts presumably established, from case histories alone are unwarranted, and lead to the type of transcendental mystery that appears to awe many of the writers in psychosomatic medicine. It is an approximate certainty that a thorough acquaintance with operational logic, as it is outlined by Pratt (5) and others (2, 3), will tend to modify considerably the interpretations attempted in this field. Undoubtedly it is true that many of the reports published have an immediate value from a remedial point of view, but good science should not be entirely discarded in the interest of pragmatic application.

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